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ABSTRACT

This final volume of a six-volume study of an elementary school called "Kensington" in a school district code-named "Milford," examines its own assumptions and procedures. After the brief, introductory chapter, which presents the methodological appendixes of the preceding volumes as a serial, chapter 2 discusses exploratory and final proposals for the study and its followup relation to an early study ("Middletown in Transition"). Chapter 3, "Ethnography as an Intellectual Process," examines the study's evolving logic; accidents, anomalies, and serendipity; the observer's role; and archival case records. Chapter 4 expands the third chapter's focus on ethnography to include separate sections on "Ethnography," "Ethnography and Historical Method," and "Educational Innovation and Life History." This final volume concludes with a chapter on "Paradigms in Debate." In order to protect the anonymity of the school district studied in such detail, pseudonyms have been used for all place names (school, school district, city, county, state) and personal names (school superintendents, school board members, teachers, students) appearing in the various volumes of this set. (JBM)

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**INNOVATION AND CHANGE IN AMERICAN EDUCATION
KENSINGTON REVISITED: A FIFTEEN YEAR FOLLOW-UP OF
AN INNOVATIVE ELEMENTARY SCHOOL AND ITS FACULTY.**

VOLUME VI

**CASE STUDY RESEARCH METHODOLOGY:
THE INTERSECT OF PARTICIPANT OBSERVATION,
HISTORICAL METHODS AND LIFE HISTORY RESEARCH**

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Case Study Research Methodology:
The Intersect of Participant Observation,
Historical Methods and Life History Research

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This report is Volume VI of a series from the project Innovation and Change in American Education--Kensington Revisited: A Fifteen Year Follow-Up of an Innovative Elementary School and Its Faculty supported by NIE Grant #G78-0074. The analysis and interpretation represent official policy of neither the National Institute of Education nor the Milford School District.

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Foreword

to

Innovation and Change in American Education Kensington Revisited: A 15 Year Follow-Up of An Innovative School and Its Faculty

(Smith, Dwyer, Kleine, Prumty)

This research is about innovation and change in American education.

It began as Kensington Revisited: A 15 year follow-up of an innovative school and its faculty, Project G78-0074, supported by the National Institute of Education. As in most of our case study research, the initial problem was buffeted about by the reality of settings, events, and people as captured by our several modes of inquiry--participant observation, intensive open ended interviews, and the collection and analysis of multiple documents. The setting was Kensington, an elementary school built fifteen years ago as a prototypical innovative building with open space, laboratory suites instead of classrooms, a perception core instead of a library and a nerve center for the latest in technological education equipment. The people were the series of administrators, teachers, pupils, and parents who worked in, attended, or sent their children to the school. Three principals have come and gone, the fourth is in his first year. Three cadres of faculty have staffed the school. The events were the activities of those people as they built and transformed the school over the years. This story we found, and we constructed, as part of a larger setting, the Milford School District which had its own story, actors, and events and which provided an important context for Kensington.

In the course of the search for the major theme about which our developing ideas and data could be integrated. "Innovation and Change in American Education", became the guiding thesis. That theme is composed of a half dozen sub themes, each of which makes up a separate volume in the report. While we believe the totality of the study has its own kind of integrity and that each volume extends the meanings of the others, we have written each as a "stand alone" piece. That is, we believe each speaks to an important domain of Innovation and Change in American Education, each draws most heavily upon a particular subset of our data, and each contains important descriptive narratives, substantive grounded interpretations and generalizations. This foreword, which appears in each volume, is intended, in a few sentences, to keep the totality and each of the pieces in the forefront of the reader's consciousness.

Volume I Chronicling the Milford School District: An Historical Context of the Kensington School

Kensington's fifteen year existence is but one small segment of Milford's sixty-five years of recorded history and one school in a district with a dozen other schools. The superintendent who built the school is just one of five individuals who have held the post. As we have told the story, we have raised generalizations regarding innovation and change, and we have presaged themes of policy, of local, state and national influences on the school, of organizational structure and process, and of curriculum and teaching. The key documents in developing the perspective were the official school board minutes. Newsletters to patrons, newspaper accounts, other records, and interviews, formal and informal, supplemented the basic documents.

Volume II Milford: The School District as Contemporary Context

In a fundamental sense, Volume II is a continuation, a final chapter as it were, to the historical context of the Milford School District. It is a long chapter, however, for the central actors and events which immediately and directly shaped the Kensington School are in place, just as the school is in place. The ebb and flow of the district, in its recent history, is brought to a particular focus, one that will illuminate the events and themes that appear in the development and change in the Kensington School over its fifteen year history and in its current status. The board of education, the superintendency, the central office staff, and their interrelationships lead toward "a governance and organizational perspective on innovation and change". Board minutes remain the central core of the data with increasing amounts of information from public documents (e.g. newspapers), interviews with central actors, and observation of meetings.

Volume III Innovation and Change at Kensington: Annals of a Community and School

After carefully examining the historical context of the Milford School District, our focus shifts to innovation and change at the Kensington School. Our search for an explanation of the profound changes that have taken place in a once innovative school, has pushed us back in time and obliged us to consider such wider topics as demography, neighborhoods, and political jurisdiction. Volume III begins by tracing origins and development of a community that became part of the Milford School District in 1949 and a neighborhood that began sending its children to Kensington School in 1964. With the opening of Kensington, the annals of the community are joined by a history of the school. As we

develop the stories of Kensington and its neighborhood in tandem, we begin to tell of the interdependency of school and community and to further our understanding of innovation and change in schooling in contemporary American Society.

Volume IV Kensington Today: Sailing Stormy Straits, a View of Education Policy in Action

An ethnographic account of the school today with particular reference to educational policy in action at the day to day school level is presented here. The major metaphor is a ship sailing through stormy straits on a perilous journey during the 1979-80 school year. Staff and students produce vivid scenes reflecting issues in racial integration, special education, discipline, and instruction in the basic subjects. Policy analysis seems analogous to the fine art of navigation.

Volume V Educational Innovators Then and Now

Crucial to any education enterprise are the people who staff the schools. Smith and Keith characterized the original faculty of Kensington as true believers. In this Volume we sketch life histories, careers, serials of the original faculty based on extended open-ended interviews (2-7 hours), comments by spouses, friends and colleagues, and various writings--books, brochures, reports, and dissertations. Patterns and themes arise in the form of "secular religion," "you do go home again," "organizational niches and career opportunities for educationists," "maintenance of educational ideology," "continuity and change in personality," and "doctoral education, a disaster for reform oriented practitioners."

Volume VI Case Study Research Methodology: The Intersect of Participant Observation, Historical Method, Life History Research, and Follow-Up Studies

Regularly in our inquiry we have produced "methodological appendices" to our research reports. We saw our efforts as clarifying the craft of research as we practiced it, ordering its evolving nature, and continuously attempting to integrate it with other ways of knowing.

This essay continues in that tradition. Specifically our mode of participant observation now has enlarged itself by a substantial historical thrust and a substantial life history or biographical thrust. In addition, our research is an instance of a special methodological stance, a follow-up or return to the setting of an earlier major study. (e.g., Middletown in Transition) In this way it takes on a time series quality with repeated observation. In doing the descriptive and analytical pieces, Volumes I through V, in reading about how others have done similar work, in talking with proponents of the various methods, we have reached for a broader synthesis of case study research methods in the intersection of these several approaches. We see all this as an important addition to the methodological literature in educational inquiry.

In summary, our research is a unique blend of approaches to the problems and issues of innovation and change in American Education. It is grounded in the multiple aspects of a single school in a single school district. As in all case studies the particular events have major meanings for the actors in the setting, but, also, we believe that these events often capture images and ideas that have relevance for other people in other times and places. Recently, Geertz has spoken of these as "experience-near" and "experience-distant" conceptions. In

each form we hope to be providing mirrors for educationists to see themselves better, that is more clearly, to be conscious of rephrased problems, and to create more viable options and alternatives. Our multi-volumed report is presented with these aspirations in mind.

1. INTRODUCTION

1.1 Methodological Appendices as a Serial

One of the most useful and important ideas from personality theory is Henry Murray's conception of "serial." (Murray and Kluckhohn, 1954) It is one of the long units, a pattern that goes on for years—a friendship, a marriage, a career, a research project. In a sense it is a theme, a thread or a strand that winds through much of a lifetime. Its potency seems to derive from several of its characteristics. It has a processual, contextualist quality, that is, it runs over time. It begins somewhere, often vaguely, can be identified because it has some coherence in form and structure, and it seems to be going somewhere, that is, it has a purpose. Because it is purposive it has a proactive flavor; it is one of those reflexive, "you can work on it" aspects of personality. Murray sees serials as major components of the ego system. Metaphorically, it has some similarities to a playwright's notion of plot in drama. This monograph, the methodological appendix, deals with a series of methodological issues related to the project, Kensington Revisited: A Fifteen Year Follow-Up of an Innovative School and its Faculty. If the pieces had been woven together more tightly it might be called a "theory of methodology." If we are not quite there, we do believe we are a step more coherent than "fragments" which seemed another likely title, at an earlier point in time.

In our work, over the years we have found the "methodological appendix" a useful and important way of clearing our own heads regarding our methods and procedures and communicating to our readers how we did what we did. We and they would then have some purposive context to

enhance the meaning of our substantive findings and construals and to enable us and them to judge, if not replicate, the adequacy of those findings and construals.

In a long project such as this one we found that our initial intentions were nudged by outside events. Mostly these seemed to be in the form of invitations--conferences, AERA symposia, book chapters and journal articles. They were opportunities to say some things about what we were doing. Mostly they were reasonably open ended. Sometimes we were able to negotiate a bit as to substance, form and length from the original intentions of the conference planner or journal editor. Invitations, opportunities, and negotiation seem to be important concepts requiring farther development in our theory of methodology. Entrepreneurial ability may be another of these necessary ideas.

A major element in the pattern of serials is the purposive aspect, they are intended to go somewhere. In our view, the doing of research is very much a craft, a creative craft that one continues to learn about. People who spin and weave, who garden, who fish, over the years accumulate a huge reservoir of knowledge that brings further meaning to the activity, that raises the quality of the products produced, that adds to the joy of the activity. Hopefully these essays have some of those features.

In organizing the essays for this final report we have blended their chronological order with a little clustering for further definition of what we are about. The first three items are only partially self explanatory. They are the preliminary and full proposals submitted to NIE in the Spring of 1977, a full six years ago, from the time of publication of this final report. The preliminary proposal

resulted from the happy confluence of participation in an AERA symposium on Case Studies, organized by Matt Miles and the arrival of the NIE announcement of the grants competition. It was the Miles' symposium that got us thinking about a return to the Kensington School. The content of those remarks indicates how the thinking began. That paper represents a fundamental link between the original project, Anatomy of Education Innovation (Smith and Keith, 1971).

Including the two proposals is a bit redundant, but it is our belief that the preliminary one, which ranked high in a group of ninety some proposals, that really won the competition for us and gave us the support to do the study. It seems "crisper" and "less clumsy" than the more developed full proposal. It may well be that for this kind of methodology, which has a responsive open ended quality, that the key idea or foreshadowed problem is highlighted. After that, and until the data begin to appear and take form, any major amplification does have an awkward quality. In the generating of ideas, the social world, the actors, and the drama of their lives are critical.

Section 3, Ethnography as an Intellectual Process, reflects our continuing concern to develop a rationale for field work in education. "The Evolving Logic..." chapter in Review of Research in Education was an invitation and a compromise. Lee Shulman and Laurence Iannaccone, at an AERA meeting, talked with Smith about the chapter. He had been trying unsuccessfully to paddle a volume of essays on field methodology. Doing a long chapter seemed an appropriate compromise since the collection seemed highly improbable. It turned out to be an attempt to legitimate qualitative field studies within AERA where the Campbell and Stanley (1963) moral stance is so dominant. The tactics involved

presenting the breadth of research already available, one personal research serial, a look at cognitive processes in case studies, and finally a move toward criterion clusters for judging field research.

The "Accidents, Anomalies, and Serendipity..." paper was presented first in November, 1978 at the Wingspread Conference on Field Methods in the Study of Schooling organized by Popkewitz and Tabachnick. The focus was on the nature and origins of the field study problem, an issue that my colleagues and students often found, from their more traditional research perspective, vague and inexplicable if not wrong. It was an enlargement of an initial idea from the evolving logic paper. It provides another large context for Kensington Revisited.

The "Observer Role..." essay was first presented in the symposium, Alternative Conceptions of Qualitative Educational Research at AERA in Boston in 1980. Shortly afterward, Smith received an invitation to do a book review of Lindblom and Cohen's Usable Knowledge. We countered with the possibility of an Essay Review based on the AERA paper. Agreement was reached and we revised the essay and published it in Educational Policy and Evaluation.

The "Archival Case Records..." is the introductory piece to a series of documents presented and discussed at Lawrence Stenhouse's Case Records Conference in York, England in September, 1980. Implicit in the account was an attempt to exemplify what Jack Hexter has called "the second record" and the good humored give and take debate over the nature of educational and social science research which was part of our relationship with Lawrence. He had put us on to Hexter's The History Primer, even though Hexter had been a long term faculty member at Washington University. Case Records as archival documents are an

unusual kind of "first record", to use Hexter's other term. The play between the first and second was part of Smith's agenda regarding the problem in case record archives.

The three essays in Section 4, Ethnography and Related Inquiry, were also the result of invitations. Ethnography appears in Encyclopedia of Educational Research (Mitzel, 1982). In an earlier and longer form it attempted even more to state several classical anthropological positions--Malinowski, Whyte and Geertz--and to locate ethnographical educational field studies into that tradition. In a sense, it developed another piece of the "Evolving Logic..." essay. Also, in writing it, Smith solicited comments from friends and colleagues in CAE, the Council on Anthropology and Education, an attempt to broaden it from just a personal statement. The last two papers relating Ethnography to History and Life History were written at the invitations of Ivor Goodson and Stephen Ball, who were part of the York Conference and they were organizers of the St. Hilda's Conference. The papers were presented at St. Hilda's College, Oxford University in September, 1982 and September, 1983. The theme of the 1982 meeting was School Subjects: Histories and Ethnographies. In 1983 the theme is Teacher's Careers and Life Histories. Both themes seemed to have extraordinary fit with two major aspects of the Kensington Revisited project.

This methodological volume begins with a short paper presented as part of a symposium at AERA before the Kensington Revisited project was conceived. Part of the agenda of that essay was a broad concern for the metatheoretical basis or the paradigmatic assumptions underlying educational research. That theme continued all through the substantive

analysis and the methodological reflections in the project. It seems fitting to conclude this volume with a brief review of Bredo and Feinberg's book, Knowledge and Values in Social and Educational Research. The thesis of the book is that the educational community is in ferment regarding its basic assumptions and that several main options are the focus of discussion. The review was written just as the Kensington Revisited project was finally being finished.

Finally, it seems that an overall research serial has an important substrand, a research methodology subserial. (We still do not have a stable language--e.g. serials, subserials, strands, threads are variations in level of generality.) The serial is not only embedded in a personality but also in a number of overlapping communities, what Smith and Dwyer called a "floating crap game" (1980, p. 443). In these instances the communities included specifically AERA, Council on Anthropology and Education (CAE), the Cambridge Evaluation Conferences, the Center for Applied Research in Education (CARE) in East Anglia, and the St. Hilda's Conferences.

By way of conclusion, it is both obvious and significant to note that the preparation of these essays was facilitated mightily by the half time project support over several years. Resources lightened teaching loads and provided times for analysis and writing. Invitations provided enough focus to bite off "chunks" of data and ideas and provided stimulating setting in which to debate with interested friendly colleagues in America, the United Kingdom and the British Commonwealth. We feel fortunate to have had a change to savor the best of this academic collegiality over a number of years.

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2. THE PROPOSALS AND AN ANTECEDENT

2.1 Kensington Reconsidered

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Presented as part of the symposium, Case Studies in the Creation of New
Schools: The Issues Reconsidered, AERA, New York City, April 7, 1977.

Part I: Early Ideas

Professor Miles tells me I have just six or seven minutes to raise issues regarding "key explanatory variables" from our study of the Kensington School. Later we'll have another six or seven minutes to reconsider some of those ideas. In such a brief time, it might be best to tell you two stories about the genesis of the research.

First, I was just coming off of a project wherein William Geoffrey and I were trying to figure out how a middle class teacher coped with a group of lower class pupils in a sixth grade elementary school classroom. We were pursuing something called teacher decision making. Lurking behind the teacher's decision making and influencing an incredible number of events in the school was a very potent clique of teachers. We didn't know whether it was the villain or hero of the piece; we did know it was an intriguing phenomenon. We wondered how it had developed. At that time, if my memory is correct, Professor Larry Iannaccone, who was running our Center for Field Studies, had been approached by the Milford School District regarding a study of Kensington, a new open space school which was beginning the next fall. Was I interested? he asked. When I found that the faculty was to be coming from all over the country, new to each other, I saw the possibilities of a study of the origins of a faculty peer group. I became very much interested and the discussion moved forward regarding intensive involvement during the next year. Shortly the faculty peer group problem disappeared in the realities of the experience: demise of a foreshadowed problem, if not explanatory variable number one.

My second story follows sequentially on the first. Right from the initial discussions we began to hear about innovation, about the

problems of "2 x 4" education, that is education between the two covers of a book and the four walls of a classroom, about individualization of instruction, and about open space architecture rather than egg crate schools. For a reasonably staid educational psychologist, it was mind boggling. Then we started to run into written documents--literally, pounds and pounds of them--building specs by a district committee, a glossy paper brochure giving a glowing prospectus of the school by ADI, the Architectural Design Institute, a dittoed document by the principal called "the Institutional Plan" and later a document by the assistant superintendent, a proposal for funds to the Olds Foundation. While trying to digest all that we soon were caught up in a month's summer workshop for the staff. The first week of that was a T group experience. I'd never been in one of those before and no one as far as we could find out had ever studied a group of people who were new to each other, whose first experience together was a week of T grouping and who then would live together for the next ten months in a formal organization. So we sat in the back, observed, took notes and tried to figure out what was happening. That too, was mind boggling.

These events were real. The "key explanatory variables" weren't forthcoming from my University of Minnesota dust bowl general psychology background (my colleague Pat Keith, who was out of a rural sociology background, was less traumatized). As we became progressively more involved with what we came to call the Merton, Blau, Gouldner, Selznick tradition we began to find a kind of order in the phenomena. This was complicated though and we had a very difficult time with finally specifying the nature of our case--1) it was an organization, 2) it was an innovative organization, 3) it was the origination of an innovative

organization, and 4) it was the origination of an evolving and changing innovative organization. We began to work with ideas such as formal doctrine, mandate and facade. We were into true belief, organizational socialization, resources, and liabilities of newness. The most illuminating of these were from a brief few pages by Selznick on the developmental problems in the origins of an organization: the selection of a social base, the building of an institutional core and the formalization of procedures. Our analysis and interpretation became very much an extension, elaboration, and interpretation of the meaning of a number of ideas in this special instance of the origination of an innovative organization. This involved a mix of foreshadowed problems, a confronting of an obstinate reality, and an attempt to specify the precise nature of the case. In short, the phenomenon itself slowly achieved a conceptual focus as did the theoretical subtheories, theoretical models and explanatory variables.

Part II: Second Thoughts

In these next six or seven minutes I would like to raise several thoughts relevant to a reconsideration of the theoretical analysis. In the Preface to Anatomy we commented:

At the universities and the research and development centers, the scholarly world of professional education and social science, of which we are a part, has failed to do justice to the complicated problems involved in originating an innovative educational organization. Investigators and theorists have not focused hard enough, long enough, nor carefully enough on the small and mundane as well as the large and important issues and problems necessary for idealistic practitioners to carry out their dreams. (pp. V-VI)

The first point I would make in a reconsideration of Kensington is that that is no longer true. The members of this symposium have produced

major papers, monographs, and books central to the major questions. In addition, groups of investigators working with and around Charters at Oregon, Goodlad at UCLA, and Herriot at ABT Associates, to mention only a few of the major efforts, have been coming to grips with the problems in the creation of new schools. That literature awaits a major codification and synthesis. There are several aspects to that literature which seem to me of crucial importance and which should be a part of that synthesis.

First, much of that data has been generated in what Mike Atkin has called "practice oriented inquiry," a kind of scholarship which "...approaches problems of the field rather than the problems of disciplines as (in an) attempt(s) to understand educational events." He contrasts this kind of inquiry with the more romantic personal testimonials in the educational literature and the more discipline oriented inquiry from fields such as educational psychology or personality psychology. He sees it as similar to the evolution of ethology. I think much of the research on innovative schools fits his perspective and that this perspective has several key implications.

Second, implicit in such an approach is a breadth and novelty of necessary concepts. As I reviewed briefly some of the work relevant to this symposium I found Gross talking about "catalyst teachers' roles," "role overload," "implementation strategy," Sarason about "before the beginning" and "confronting history," "formation of the core group" and "myth of unlimited resources", and Fullan on "active user role," "effective educational change processes," and "reform." That the list can be made longer and stranger is obvious. Practice based inquiry gets one into funny novel phenomena which do not fit the usual social science

disciplines in which most of us were trained. My point is simple--I think we must reconstrue the kinds of theoretical backgrounds and training programs in social science necessary for understanding educational phenomena. At a minimum this is asking for a broad social science approach to educational problems. The people struggling with these issues are part psychologist, part sociologist, part political scientist, and part a little of everything else. Not a happy state of affairs in general and even less so if one's tolerance for ambiguity is not high.

My next point, is really a complex criticism of the prior point. The minimum need for an interdisciplinary social science orientation is a misconstrual. The alternative I want to argue for in the remaining minutes is toward the development of a theory of education. The structure or paradigm for such a theory has several ingredients, and these suggest ways I would reconsider or reinterpret the "key explanatory variables" in our analysis of Kensington. First, I would continue the practice based inquiry orientation for it raises real and important problems in the lives of children, teachers, administrators, parents, and other key actors. The problems are not pseudo issues. Second, educational theorists need to begin to lay out their minimum terms, their borrowed terms and their derived terms. For instance, are there concepts such as education, teaching, curriculum, learning, steering groups, frame factors, recitation, that "belong" to education and not to other disciplines? Third, I would argue for miniature and middle range theories of major problems areas--such as the creation of new schools. In effect, in the Kensington analysis we were trying for these regarding such topics as team teaching, democratic administration,

and innovative teacher/pupil relationships. These analyses should be built upon the same primitive terms and expanded where necessary to handle the phenomena under consideration. In that way they would be both cumulative and interdependent. We kept reaching for coherence; I don't think we really understood the synthesis needed. Fourth, I would argue that these miniature and middle range theories of educational phenomena should coalesce into a general theory of education. And therein lies a further major point. When one starts trying to define "education," or "teaching" as philosophers such as Peters and Scheffler do, one finds a further interesting anomaly. The basic, primitive, root concepts contain normative/ethical aspects and they imply a theory of action. In effect, I would argue that this educational theory is not just a scientific theory but a blend of ethical and scientific thought. This argument is long and involved and one I don't really understand but one I find appealing. My acquaintance with it arose in debates with more philosophically oriented educators in New Zealand; it stems from the philosophical controversies in England and the United Kingdom among O'Connor, Hirst, and Struthers as to whether educational theory is mostly scientific theory, mostly ethical theory, or a blend of the two.

In summary, my reconsideration of the Kensington analysis would involve me in what might be called a meta theory, the context of the theoretical interpretation. I think the data remain significant. I think we made a reasonable start in the theoretical interpretation. I'm struck with similarities in the findings of many other studies. I think the next step is toward a more general, codified and potent theory of

education. I think such a theory will be a blend of science and ethics.

I think it will be a theory of the practical and a theory of action.

All this I find to be a large and interesting agenda.

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2.2 Preliminary Proposal

NATIONAL INSTITUTE OF EDUCATION

Program of Research Grants on Organizational Processes in Education

PROPOSAL FOR RESEARCH PROJECT

COVER SHEET

(Suggested Format)

Title of study Kensington Revisited: A 15 Year Follow-Up of an Innovative School and Its Faculty

Type of proposal: X Preliminary Full Small Grant

Total Project Period: From 1/1/78 Through 5/30/80

Principal Investigator:

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Total Federal funds requested: _____

Federal funds requested for first 12 months: _____

APPLICANT CERTIFICATIONS:

1. To the best of my knowledge and belief, statements in the attached proposal are true and correct, the document has been duly authorized by the governing body of the applicant, and the applicant organization will comply with NEW and NIE terms and conditions in effect at the time of award.

Date _____

Institutional official

2. The undersigned agrees to accept responsibility for the scientific and technical conduct, of the research project and for provision of required reports if a grant is awarded as a result of this application.

Date _____

Principal Investigator

IIA. Description and Rationale

The intent of this study is the development of basic knowledge of the middle to longer term impact of innovation in American education. Most of the recent literature on innovation focuses on the immediate problems and processes of school change and does not attend to the longer term consequences (e.g., CERI 1973, Fullan 1972, Sarason 1971 and 1972, Rogers and Shoemaker 1971, House 1974, March and Simon 1958). Over a dozen years ago we carefully observed, described, analyzed and reported, in Anatomy of Educational Innovation (Smith and Keith 1971), the first year in the life of the Kensington Elementary School in the Milford School District. As reported, Kensington represented a major attempt by a local school district to develop an innovative elementary school program. An open space building was designed and built. Staff was recruited from all over the Midwest. A comprehensive change strategy, what we came to call "the alternative of grandeur", was devised and implemented. A month long faculty summer workshop, including a week of T grouping, was instituted to provide organizational socialization. A variety of specific innovations - democratic interpersonal relationships (pupil-teacher, teacher-principal), individualized curriculum and instruction (no textbooks) and team teaching were interrelated. The present project, by one of the original investigators, proposes a dual follow up:

- 1) locate, observe and interview the two dozen key administrators and teachers who originated the school.
- 2) return to the school and by means of ethnographic/participant observation procedures observe, describe and analyze the present workings of the school.

The intent would be to capture each part of the school and its original faculty at a second period in time, to make comparisons, and to draw inferences about innovation and its effects on the lives of a small group of people. Informal contacts with the individuals who were part of the school suggest that for some of the original participants the year or two at the school was a peak experience. Other casual data from the school suggests that Kensington as an innovative school passed out of existence and reverted to the "old Milford."

In regard to the first problem, the follow up of the original faculty members, the first round of questions/foreshadowed problems would include:

- 1) Where did the faculty go? What have they done? What are they doing now?
- 2) What role did the Kensington experience play in their lives? How do they see the experience now?
- 3) What is their general educational perspective now? What is their point of view about educational innovation?

In this part of the project, the implications of the data would be examined in terms of a theory of personality. In particular, such key conceptions

as organizational context, careers, serials, beliefs, values, and life span psychology would be explored (e.g., Argyris 1957, Holland 1973, Murray and Kluckhohn 1961, Rokeach 1975, Baltes and Schaie 1973, White 1952). The focus would be to place the issues of educational innovation into the broader context of the individual's life. The most relevant comparison groups of "teacher-in-general" are from Waller (1932) and Lortie (1975).

The second problem, revisiting the Kensington School per se, amounts to doing a contemporary version of Anatomy of Educational Innovation. The central focus would be a descriptive/analytical account, that is a theory of elementary school structure and function. Within this, such concepts as formal doctrine, institutional plans, governance, administrative leadership, faculty peer groups, teaching, curriculum, teacher pupil relationships, pupil activities and learning and the nature of physical plant and facilities would be raised. These realities would be interplayed with the broader district context - other elementary schools, articulating with the junior high school, central office positions and incumbents, and the local community of parents and patrons.

Each of these items would be paralleled to the account of the early years. Similarities and differences would be examined. A paradigm for systemic comparisons would be generated. For instance, "minor" changes in architecture - enclosed hallways, permanent room dividers, etc. would be isolated, described and interconnected with social structures and processes.

The substantive problems in a follow up study such as this imply several important issues in a theory of methodology of the social sciences. Without elaborating them in detail now, two of these are 1) issues in the rights of human subjects and 2) the relation of historical methods to empirical social science methods. In a sense, returning to the lives of the Kensington staff can be construed as a major invasion of their privacy or as a positive opportunity to recapture a major earlier period in their professional lives. Empirical data on such issues are very limited. Regarding the second issue, when one spans fifteen years between two studies, one seems to be moving toward the domain of history. A number of issues in the relation of oral history to participant observation and interviewing (e.g., Baum 1971, Becker 1970, Dexter 1970, Merton et al. 1956) and between historical description and explanation and social science description and explanation (e.g., Hempel 1942, 1965, Scriven 1959, Dray 1957 and Hook 1963) seem open for analysis. Such consideration should help undergird the substantive position.

IIB. Procedures and Methodology

1). Overview: The parts of the investigation interlock in several ways. Briefly I would interplay time lines and structures this way over 2½ years.

1.1) First semester (1/78-5/78) begin the locating of people, address the problems of rights of human subjects, train the two assistants, refine the foreshadowed problems, build mutuality in district relationships.

- 1.2) Academic Year I (9/78-5/79) carry out the systematic interviewing of former faculty described as Problem I and carry out the full year of participant observation of the school Problem II. Begin the initial parts of the analysis.
 - 1.3) Academic Year II (9/79-5/80) major analytic effort and the writing of final report. Also, if necessary return to the school and/or further interviews of former faculty to refine interpretations. The write-up conceivably could involve one book length monograph or two shorter monographs on each problem area treated independently and one long integrative essay.
- 2) More specifically, the participant observer study of the present day Kensington would follow upon a long tradition of our earlier work. For brevity I would refer the reader to our previous accounts (Smith and Geoffrey 1968, Smith and Keith 1971, Smith and Pohland 1974, 1976, Smith and Schumacher 1972). Over the years this work has been guided by the work of a number of investigators (e.g., Becker 1958, 1970, Bruyn 1966, Denzin 1970, Glaser and Strauss 1967, and Zetterberg 1965).
 - 3) The procedures regarding the follow-up study represent a major extension and elaboration of our case study methods. Several models are available (e.g., Terman and Oden 1947, Sears 1977, Skeels 1966, Nesselrode and Reese 1973). Specifically, several problems loom up as large. The first is locating the people. While this will be harder than I suppose, I have been in contact with several over the years, several have taken Ph.D.'s and are in colleges and universities, some have kept up with each other. By telephone, letter, and inquiries at old addresses I would propose to track them down. Skeels suggests three essential qualities in this, "flexibility, ingenuity, and tenacity (p. 28). Assuming their interest, willingness, and informed consent, I propose to visit each for two to three days of conversation, interview, and observation. The format of the interview would be a blend of narrative, open-ended questions, and focused questions. The content would reflect the concepts, problems, and issues listed earlier. Models would be similar to Gross et al.'s (1957) eight hour interview of school Superintendents, Merton et al.'s (1957) focused interview, and the procedures of oral history (Baum 1971 and Dexter 1970). As much as possible of this would be taped for later analysis (e.g., Easley 1974, Smith and Brock - in process). The observations would be of the individuals in their current organizational contexts. Their teaching, their administering, their current collegial relationships, and their professional activities would be observed and noted. Further, copies of their professional writings would be collected. Finally, we would readminister two inventories of attitudes and beliefs, the Minnesota Teacher Attitude Inventory (Cook et al. 1951) and the Teacher Conceptions of the Educational Process (Wehling and Charters 1969) which were first given 15 years before. The issues of validity of data and interpretations would be approached in three central ways. As indicated, I would talk with the teachers in the context of their current positions and some of their current colleagues, that is, play off the interview against their current teaching, administering, and writing.

This is similar to the triangulating and multi-methods we have used in our participant observation research and builds on the ideas of Denzin (1970), Campbell and Fiske (1959), and our extrapolations (Smith and Pohland 1974, 1976) of those efforts. The second technique would involve cross checking of comments among the people as we explore - What's happened to--? - in the interviews. Casual data from recent contacts with some suggests this is both feasible and potent. Third, the rapport and personal relationships of the earlier period (e.g., the many accounts in Anatomy) was/is such that the faculty will be honest and open. The researcher and his role in the past were perceived as careful and honest.

- 4) Blending the two major aspects of the study, the follow-up and the study of the current structure and processes of the school would be a sampling and interviewing of cohorts of individuals who were in the school between 1964-65 and 1978-79. Such data would help clarify the dynamics of the change in the school over the fifteen years.
- 5) A further idea (suggested by Per Dalin of OECD) would be to bring the original group back to Kensington for a two day reunion at the school. This would blend a variety of issues regarding interpersonal relations, points of view, and affect. I have some ambivalence about this. The decision might have to await contact with the faculty and be contingent on their reactions. (At this point no budget allocations have been made.)
- 6) In conclusion, for a number of reasons of personal preference regarding research activities, I would plan to be involved in the day-to-day nitty gritty of all phases of data collection, analysis and write-up. In addition I would hope to have two assistants, one in each phase of the project to increase coverage, to provide another view, to increase the theoretical depth, and to help with the innumerable problems along the way. Some part of each study would be "broken out" or "chunked" to become their dissertations. Over the years we have had good success with this (e.g., Klein 1966, Pohland 1969, Schumacher 1974).

11C. Summary

Overall, and in brief, the project is aimed toward 1) basic knowledge, 2) innovation in local school organizations, 3) the interplay of organizations, careers and personality, and 4) a relatively long (15 year) time perspective. The methods grow out of a strand of research labeled case studies, field studies, ethnography of schools, and participant observation; procedurally this involves lengthy interviews and intensive observation in the context of a particular time, setting, and group of colleagues. To the best of my knowledge, such a project is unique in the literature of educational organizations. The results should contribute to the understanding of the many issues in innovations in school organizations.

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III. Description of Facilities and Arrangements

- 1) Initial discussions are underway with the Superintendent and staff of the "Milford School District". The district is both interested and supportive. The tenor of this is caught best in the superintendent's comment, "I'd like to be with you when you walk through the building again for the first time", which arose in our discussions of changes in the building physical structure.
- 2) Contacts have been made with several persons from the earlier study. They see the problem as important, are willing to participate, and think the others would be also.
- 3) Both Pat Keith and Paul Kleine, my colleagues from the earlier study, have read an early draft of this proposal, have made critical suggestions regarding substance and method, and have agreed to serve as consultants to the project. Professor Keith holds a joint appointment in Sociology and Education at Iowa State University; Professor Kleine is Chairman of the Education Department at University of Wisconsin, Parkside.
- 4) Washington University has a history of academic freedom and support of inquiry. The Graduate Institute of Education has an excellent research oriented faculty and graduate student population. The collegial support is seldom matched elsewhere. Recently an inter-departmental and interschool program in Organizational Behavior has been initiated at Washington University. I'm on the Administrative Committee of the program.

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To the best of my knowledge and belief, statements in the attached proposal are true and correct, the document has been duly authorized by the governing body of the applicant, and the applicant organization will comply with NEW and NIE terms and conditions in effect at the time of award.

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2. Abstract

This research proposes to develop basic knowledge regarding innovative school organizations. Its central thrust is a fifteen year follow up of the innovative Kensington School and its original faculty. This involves a complex interrelation of organizational theory and field research methods. The principal investigator of the proposed research was also the principal investigator of the original study, Anatomy of Educational Innovation (1971). The return to the school attempts to answer two questions - What is the current structure of Kensington as an educational organization, that is, has the school reverted to the old Milford as was predicted in the original monograph? Second, what interpretation/explanation can be made of the presumed changes between 1964 and 1979? Methodologically, this will involve a special kind of case study, a mix of ethnography and recent history. Participant observation, interviews/oral history, and primary documents eg. local newspapers, school bulletins and school records will be used.

The follow up of the original faculty links personality theory of several kinds with organizational theory. From a common sense framework, the questions are simple: What has happened to the original staff? How do they perceive the impact of the Kensington experience on their professional lives? The key concepts are life span/development perspectives, career lines, organizational positions. By means of intensive interviews, observations of current work-teaching, administering etc. and document analysis, we hope to chronicle and to interpret the fifteen year period.

In short, the problems, the analytic/interpretive frameworks, and the procedures build upon and extend our prior work.

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4. Research Objectives

Introductory Context

In its simplest yet most general form this research is a proposal for a fifteen year follow up of a highly innovative school and its original faculty, now scattered across the country. Such a research objective poses two immediate subproblems.

1. locating the former faculty and administrators and through observation and interview obtaining a view of their later careers and their views of educational innovation and change.
2. returning to the Kensington School and the Milford District for a special kind of ethnographic case study, that is, a descriptive/analytical account of the current status of the school plus an historical chronicle and interpretation of the process of change in the school over the decade and a half.

The two problems are embedded in a unique empirical and methodological context. The first year of the school was intensively studied, a long monograph, Anatomy of Educational Innovation, presented the results. Such a benchmark seems rare if not unique. The inquiry will be conducted by one of the original investigators; each of the other investigators involved in the original study has agreed to serve as consultants on the proposed project. Finally, our most recent research, Science Education in the Alternative Schools: A Kind of a Case Study (Smith, 1977) involved an attempt to take seriously the historical dimension as well as the contemporaneous structure and dynamics of a school district and to develop a theory of educational change in the context of the science education program.¹ In short the proposed research is part of an evolving and cumulating educational research program.

The Problem from the Laymen's Perspective

At its most common sense level the problem of this investigation is simply an answer to the question, What has happened to the innovative Kensington School after 15 years? In what ways is it similar or different? For instance, is the building still as beautiful as it was? Have the carpets, an issue of contention a decade and a half ago, held up? Did the open outdoor play shelter with its problems of wind, rain, dirt and heating get closed in to make a multipurpose room? If so, what did it do to the ideology of "multi-useless rooms"? Have the large open space loft areas been maintained or have walls been built to convert the open instructional areas into more self contained classroom space? Within the curriculum and teaching domains, has team teaching and individualized instruction reached new heights and become stabilized standard modes of instruction? Or, is there some latent irresistible potency to the self contained classroom? Have the technological promissary notes of the "nerve center" been realized with full play to centralized but

¹ This research was one of the eleven case studies in the NSF project, Case Studies in Science Education. The principal investigators are Professors Robert Stake and Jack Easley of the University of Illinois.

individualized audio, visual, computerized instructional systems? Has the perception core continued with its early promise and become an educational materials center with multiple and varied resource for pupils and teachers? Or is there a potency - economically, organizationally, instructionally, and educationally - to the textbook, that its return has been irresistible? Finally, the original Kensington doctrine was a radical manifesto for democratic control of education. The pupil was to be in control of his/her learning, the teacher in control of his/her teaching, and the principal in control of his building vis-a-vis the district. Stated simply, what is the current resolution of the governance issue? How did it come about?

In regard to the follow up of the original faculty members, the first round of questions/foreshadowed problems would include:

1. Where did the faculty go? What have they done? What are they doing now?
2. What role did the Kensington experience play in their lives? How do they see the experience now?
3. What is their general educational perspective now? What is their point of view about educational innovation?

While such questions can go in several directions, the theme that will be returned to continually, is the meaning for schools-their structures and processes.

These "common sense"/"laymans" issues and questions are spoken to most forcefully in what we have usually called a descriptive narrative, account of the day to day events in everyday prose.² As the chronicle and narrative are developed, the issues of interpretation enter implicitly and soon the observer/analyst has taken a position within theories of social change, a problem which will be addressed shortly (Section 4.4).

Miniature & Middle Level Theories of Education

For some time, the positions of Merton (1957) and Zetterberg (1965) on middle range and miniature theories in social science have had considerable appeal as working strategies for research. They will continue to be guides for this investigation. As such, the strategies will take the concrete specifics of the Kensington School and move them to hypotheses of a more abstract & general sort. Several items from the earlier analysis suggest the directions this might take. Once again these are illustrative issues, foreshadowed problems, for the present investigation.

Organizational Structure and Process: Within organizational change the role of leadership is important. In the earlier analysis we argued that the leaders

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In the methodology we will raise some of the problems of the "narrative", especially in the context of White's (1963) provocative analysis.

of the Milford District and the Kensington School adopted a broad strategy of innovation - what we called "the alternative of grandeur", the alteration of multiple components.³ The organizational change literature suggests other possible views, for example, Etzioni's "gradualist strategy."

Insert Figures 1 and 2 about here

In the proposed investigation we will examine these positions in both their "espoused" and "in use" forms (Argyris & Schön, 1974) with the original data from the former staff of Milford/Kensington and with the new data from the current staff of Milford/Kensington. The environmental/contextual elements in the antecedents, the interdependence with the content of the formal doctrine, the social structures and processes of the school and district are implicit domains of interest. A carefully drawn historical chronicle of the events between the two periods will be used to trace the flow of change.

In the earlier study, we identified the Kensington School as a "protected subculture", a temporary system in the overall district change strategy. Is Kensington still so regarded? If not when did it lose that special status? Where does it fit in current district policy? Why? What are the consequences?

Insert Figure 3 about here

In short, revisiting the Kensington School per se, amounts to doing a contemporary version of Anatomy of Educational Innovation. The central focus would be a descriptive/analytical account, that is a theory of elementary school structure and function. Within this, such concepts as formal doctrine, institutional plans, governance, administrative leadership, faculty peer groups, teaching, curriculum, teacher/pupil relationships, pupil activities and learning and the nature of physical plant and facilities would be raised. These realities would be interplayed with the broader district context - other elementary schools, articulating with the junior high school, central office positions and incumbents, and the local community of parents and patrons. Each of these items would be paralleled to the account of the early years. Similarities and differences would be examined. A paradigm for systemic comparisons would be generated and implemented.³

Personality Theory and Educational Organizations: A career is a "course of a person's life, especially in some pursuit." As such it is a long time unit in a theory of personality, a serial in Henry Murray's conceptual framework (1938, 1951, 1956). The intent of this part of the analysis will be to

³Further discussion of the paradigm appears in the methodology section of the proposal.

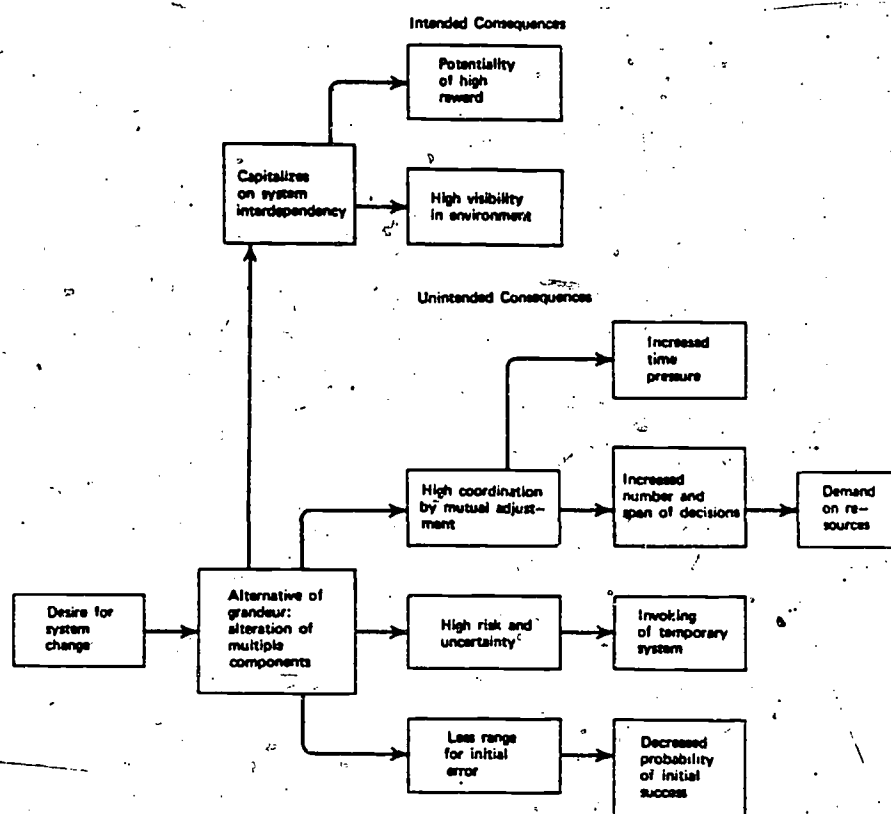


Figure 1. The implications of the alternative of grandeur as an innovation strategy.
(from Smith and Keith 1971, p. 319)

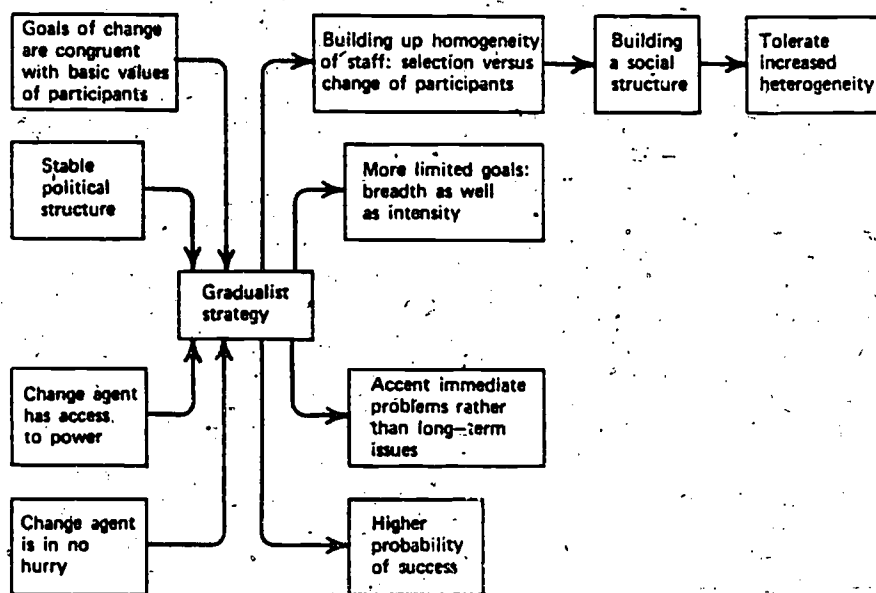


Figure 2. The determinants and limits of the gradualist strategy
 (after Etzioni, 1966, pages 64 to 78).
 (from Smith and Keith 1971, p. 372)

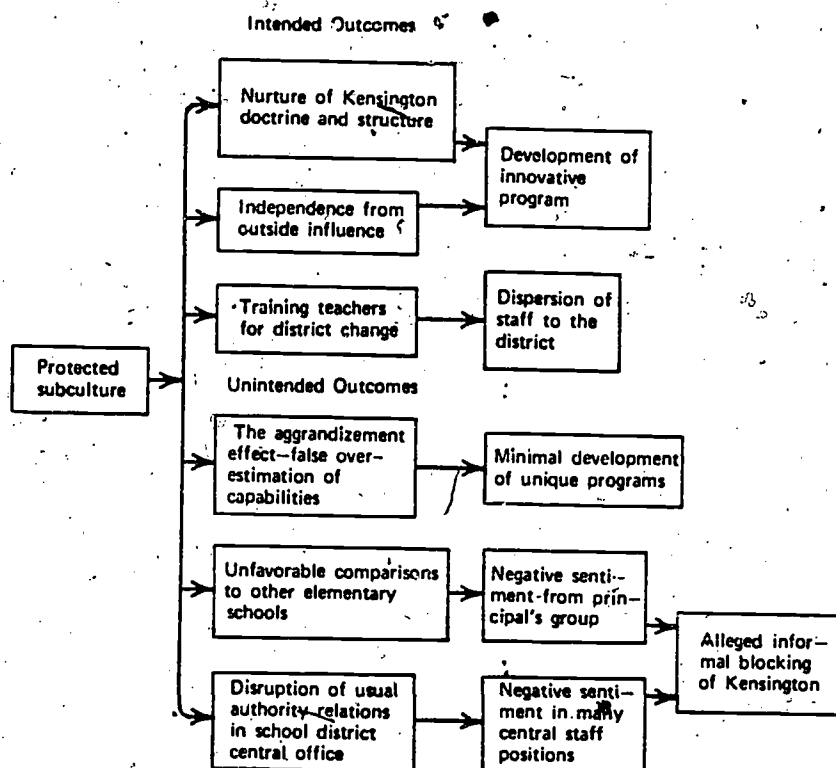


Figure 3. The consequences of the protected subculture as a temporary system.
 (from Smith and Keith 1971, p. 379).

develop a careful chronicle of the educational positions held by each individual, the noneducational positions, educational training, other major events (eg. illness, marriages, divorce, retirement). Woven into this will be more subjective reports of decisions, plans, strategies, affect and feeling. The thrust will be toward a grounded theory of personality and educational careers (Becker 1970, Thompson, 1976). The unique thrust will be the commonality of participation in a highly innovative educational experience. Our earlier analysis placed heavy emphasis on true belief, as Hoffer (1951) had developed the idea. Now we can see that in the context of another dozen - fifteen years. Casual contacts over the years indicate that some members of the faculty went on to other innovative schools, some went on to doctoral training programs, others became professors at colleges and university's, and at least one has left professional education altogether. Figure 4 is a tentative schematic outline of the possible play between data and ideas for capturing these phenomena.

Insert Figure 4 about here

As these accounts are cumulated over the individuals who have been a part of Kensington, they will be searched (compared, contrasted, clustered) for latent patterns and regularities. This is in keeping with our earlier work in qualitative analysis of data (Smith 1974, Smith & Pohland 1976).

In this domain of careers and personality our earlier report raised "inexperience" as a key analytical conception in both the functioning of the school as an organization and the lives of the individual faculty members. Figure 5 is from that analysis.

Insert Figure 5 about here

At this point, a dozen years later, the concept will be reanalyzed in terms of the current positions of the former faculty and their perceptions of then and now. For the former administrators, the place of "experience" in their theories of administration, organizations, and education change will be explored. Contrasts with "present day Kensington" and with "Kensington - over-the-years" will be made.

The professional training of educationists (and I use that term non-perjoratively, I consider myself one of the class) is a tangled set of affairs, if one can believe Sarason (1962) who calls the preservice phases "an unstudied problem", the problems of the TTT programs, and the current concerns over teacher centers (DeVaney 1977). In our earlier analysis and interpretation we developed a number of hypotheses regarding "the unfinished doctorate: all but the dissertation." Figure 6 is reproduced from Anatomy.

Reactions to the
Experience (Decisions,
Strategy, Affect, etc.)

Non Educational Positions

Chronicle of Educational
Positions

Training
Experiences

Other Major Events

Teacher/Administrator
at Kensington

Current Positions

Superintendent

Principal

Professor

Teacher

(elem/sec)

Retired

Other

(Pre Kensington)

1964-65

(Post Kensington)

1978-79

Figure 4. Schematic representation of educational careers over time

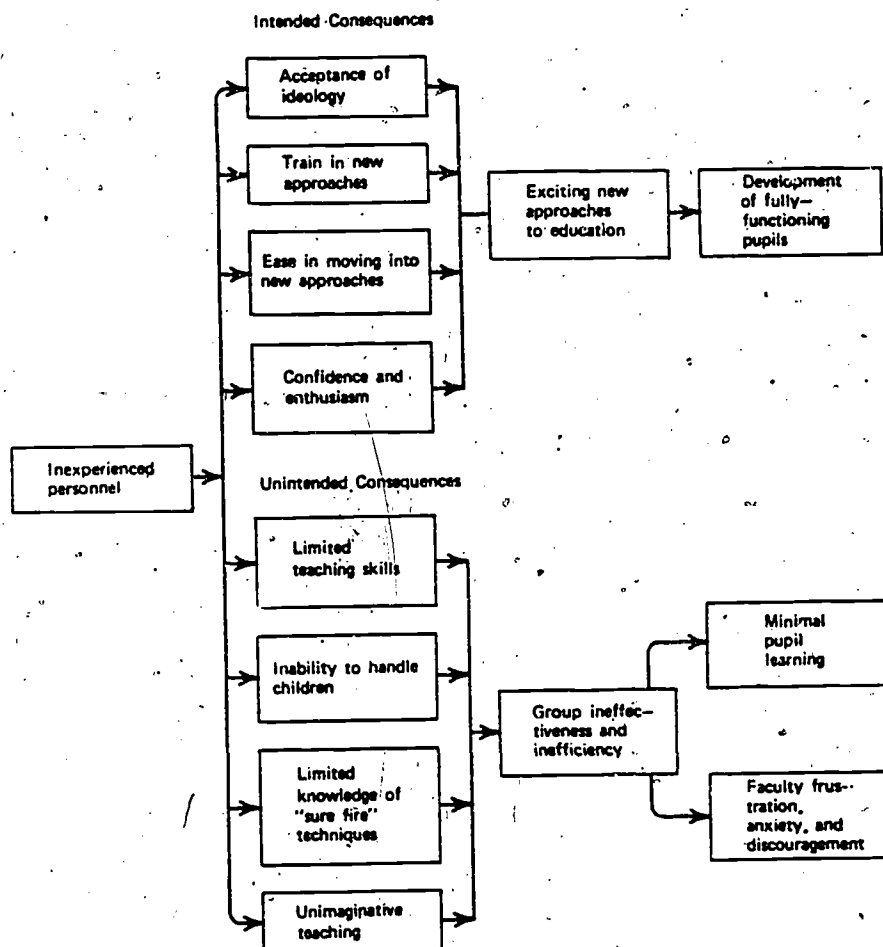


Figure 5. The implications of inexperience in teaching.
(from Smith and Keith 1971, p. 395).

Insert Figure 6 about here

The intent at this point will be to try to disentangle the nature and significance of formal professional training in the professional lives of this group of educationists. In short, in this part of the project, the implications of the data would be examined in terms of a theory of personality. In particular, such key conceptions as organizational context, careers, serials, beliefs, values, midlife crises and life span psychology would be explored (e.g., Argyris 1957, Holland 1973, Erickson (1963, Sheehy 1976, Murray and Kluckhohn 1961, Rokeach 1975, Baltes and Schaie 1973, White 1952). The focus would be to place the issues of educational innovation into the broader context of the individual's life. The most relevant comparison groups of "teachers-in-general" are from Waller (1932) and Lortie (1975). The overriding concern is a conception of personality sufficient to understanding and working in educational organizations.

Cultural, Organizational, and Social Psychological Change
Theories: An Educational Test Case

Theoretically and empirically, Kensington Revisited is a web of interlocking elements, strands, people, and events. One of the most overarching frameworks of what this part of the study hopes to inquire into is represented in the miniature theory presented as Figure 7 which appeared initially in the "Introductory Epilog" of Anatomy.

Insert Figure 7 about here

At the time the initial report was written, Kensington was "reverting to the old Milford type". That was not intended then, nor now, as an evaluation of either Kensington's or Milford's policies, programs, organizational structures, or community. Rather it was a statement of what seemed to be happening. This proposed investigation will take this model as one starting point and seek data - in records, documents, interviews on the degree to which it represents "reality" and the degree to which it needs modification. In the course of this we should begin to elaborate in a contemporary setting what anthropologists speak of as a theory of cultural change. (Hatch, 1973, Kuper 1973).

Concurrently, we will attempt to develop a position more in accord with Thompson (1965), Rushing & Zald (1976), Dill (1962) who stress the organization as an intentional adaptive entity, an open system in an ever changing environment. An item from the earlier analysis suggests the breadth and variety of possible starting points.

Insert Figure 8 about here

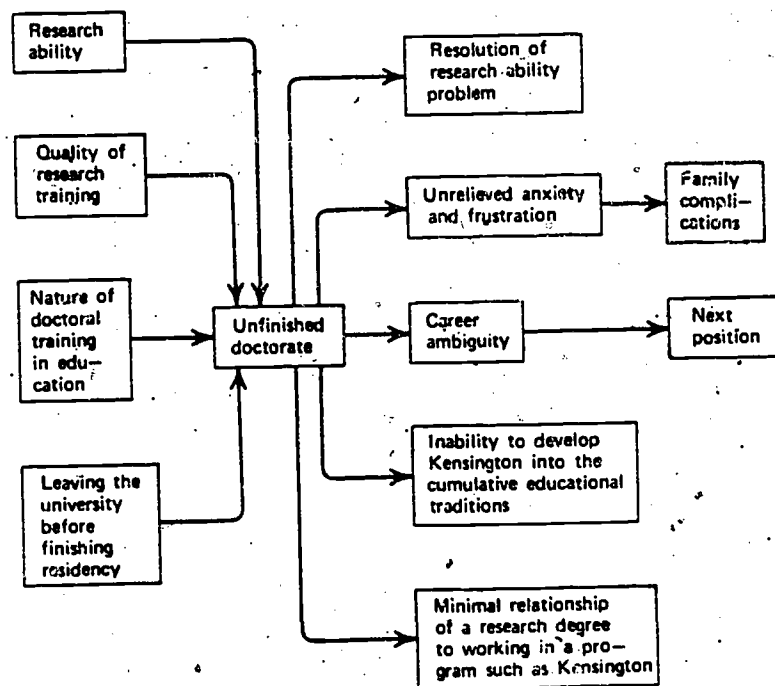


Figure 6. The problems associated with "all but the dissertation."
 (from Anatomy 1971, p. 352).

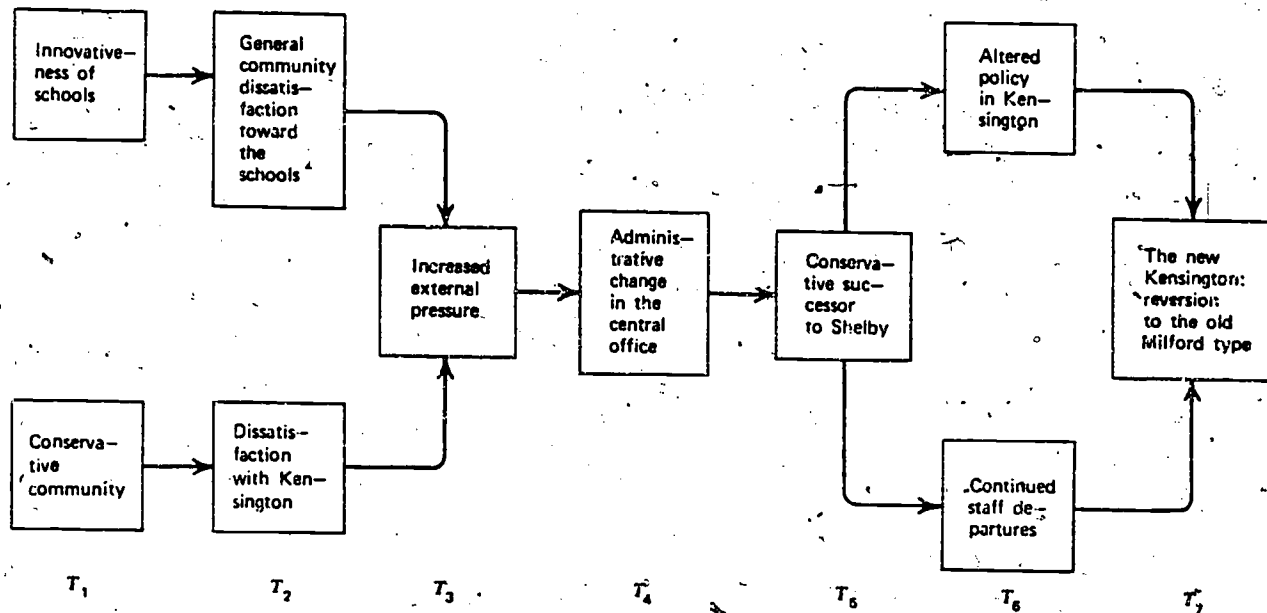


Figure 7. The social context of Kensington's administrative change.
(from Smith and Keith 1971, p. 16)

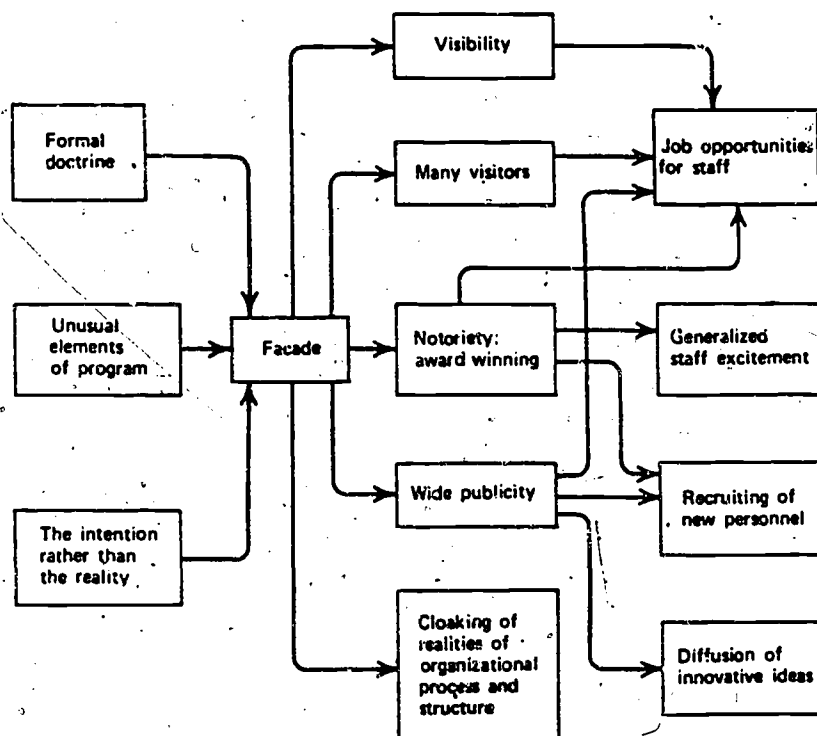


Figure 8. The implications of the facade.
(from Smith and Keith 1971, p. 51)

The facade was an important version of the formal doctrine of the Kensington School.

Thirdly, in our study of the Alte Schools (Smith 1977) we were moving toward a more interpersonal/social psychological stance as we described and analyzed the interrelated elementary school changes in the introduction of I.P.I. Math (Individually Prescribed Instruction in Mathematics) and the development of LRC's, Learning Resource Centers, in the Alte School district. In the summary of that report there were several paragraphs on "a paradigm of district structure and process".

Some years ago a perceptive social scientist argued that one should focus one's observations on conflict, not for its obvious sensational properties but rather for its proclivity in bringing to the fore latent structures and processes. The description and analysis of IPI and the LRC's seemed to do that very well. The concluding metaphor is that:

a school district is a quasi-stationary social system (equilibrium?) of contending individuals and groups with different agendas, points of view, domains of interest, and power.

That may not be "news" either, although a review of texts in educational administration and organizations might be revealing. The full implications of the position are not clear, but several suggest themselves, mostly in the form of "A school district is not just..."

For instance, the Alte District is not just a

- 1) political system--although it is assuredly that in some respects,
- 2) formal organization with agreed upon objectives and rational means to reach those although there are elements of that, too,
- 3) community of scholars although it has many scholars (both faculty and students) who do live together reasonably well (with a few notable exceptions),
- 4) coercive institution even though pupils are required by law to be there, nor totally a remunerative organization even though teachers are paid to be there,
- 5) a learning system of inputs, through puts and outputs although pupils do arrive at five years, leave at 18, and in the interim learn well, by most criteria,
- 6) a professional society even though the faculty and administrators are very much professionals in the best sense of that term,
- 7) a mechanical system of replaceable parts and pieces although incumbent teachers do fill certain positions that will remain after they leave,
- 8) an organic system that grows through God's help and some simple human nurturance, and
- 9) a legal system although the district is enmeshed in federal, state, and local laws.

In effect, we are arguing for a point of view of a system of self determining actors with individual points of view, prevailing interests, and patterns of talents who coalesce into factions and subgroups around issues of the moment. Each of the "nothing but's" is an important but partial way of stating the general position. Further, we are saying that many of the facets of science education in the Alte Schools, and perhaps in some other places, cannot be understood nor explained without such a paradigm. (Smith 1977 pp. 110-111).

As these positions are elaborated in detail and as their important points of contrast are identified, they will be put up against a reanalysis of the original data, the new data from the ethnography of the school and its community setting, and the chronicle of the changes over the years.⁴ This should produce a provocative testing of contrasting theoretical positions.

Toward A General Theory of Education

Recently, this investigator participated in an AERA symposium, "Case Studies in the Creation of New Schools: The Issues Reconsidered".⁵ While the presentation has the colloquial quality of an oral presentation it states succinctly a major quest of the present investigation, hence it's quoted in some detail.

The first point I would make in a reconsideration of Kensington is that (a dearth of information on the genesis of innovative schools) is no longer true. The members of this symposium have produced major papers, monographs, & books central to the major questions. In addition, groups of investigators working with & around Charters at Oregon, Goodlad at UCLA, & Herriot at Abt Associates, to mention only a few of the major efforts, have been coming to grips with the problems in the creation of new schools. That literature awaits a major codification and synthesis. There are several aspects to that literature which seem to me of crucial importance and which should be a part of that synthesis.

First, much of the data has been generated in what Mike Atkin has called "practice oriented inquiry", a kind of scholarship which... approaches problems of field rather than the problems of disciplines as attempts to

⁴In effect we are reaching for an ethnographic methodology for approaching Popper's principle of falsification. An early sketch of this appears in Essay 5, "Some not so random thoughts on doing fieldwork" (Smith 1975); fuller treatment will appear in "The logic of ethnography and other case studies" (Smith, in process).

⁵This symposium illustrates several other points relevant to the present proposal: 1) in thinking about what original ideas could be presented in the symposium the overwhelming reaction was a lack of data on the school and what had happened to the faculty, in effect the genesis of the current proposal 2) the integration with related literature 3) one strategy for the continuing dissemination of the results of the investigation.

understand educational events". He contrasts this kind of inquiry with the more romantic personal testimonials in the educational literature and the more discipline oriented inquiry from fields such as educational psychology or personality psychology. He sees it as similar to the evolution of ethology. I think much of the research on innovative schools fits his perspective and that this perspective has several key implications.

Second, implicit in such an approach is a breadth and novelty of necessary concepts. As I reviewed briefly some of the work relevant to this symposium I found Gross⁶ talking about "catalyst teachers roles", "role overload", "implementation strategy", Sarason about "before the beginning" and "confronting history", "formation of the core group" and "myth of unlimited resources" and Fullan on "active user role" "effective educational change processes" and "reform". That the list can be made longer and stranger is obvious. Practice based inquiry gets one into funny novel phenomena which do not fit the usual social science disciplines in which most of us were trained. My point is simple - I think we must reconstrue the kinds of theoretical background and training programs in social science necessary for understanding educational phenomena. At a minimum this is asking for a broad social science approach to educational problems. The people struggling with these issues are part psychologist, part sociologist, part political scientist, and part a little of everything else. Not a happy state of affairs in general and even less so if one's tolerance for ambiguity is not high.

My next point, is really a complex criticism of the prior point. The minimum need for an interdisciplinary social science orientation is a misconstrual. The alternative I want to argue for in the remaining minutes is toward the development of a theory of education. The structure or paradigm for such a theory has several ingredients, and these suggest ways I would reconsider or reinterpret the "key explanatory variables" in our analysis of Kensington. First, I would continue the practice based inquiry orientation for it raises real and important problems in the lives of children, teachers, administrators, parents and other key actors. The problems are not pseudo issues. Second, educational theorists need to begin to lay out their minimum terms, their borrowed terms and their derived terms. For instance, are there concepts such as education, teaching, curriculum, learning, steering groups, frame factors, recitation, that "belong" to education and not to other disciplines? Third, I would argue for miniature and middle range theories of major problem areas - such as the creation of new schools. In effect, in the Kensington analysis we were trying for these regarding such topics as team teaching, democratic administration, and innovative teacher/pupil relationships. These analyses should be built upon the same primitive terms and expanded where necessary to handle the phenomena under consideration. In that way they would be both cumulative and interdependent. We kept reaching for coherence; I don't think we really understood the synthesis needed. Fourth, I would argue that these miniature and middle range theories of educational phenomena should coalesce into a general theory of education. When one

⁶The symposium was organized and chaired by Matthew Miles; participants were Neal Gross, Barry Gold, and Michael Fullan; critics were Seymour Sarason and David Cohen.

starts trying to define "education", or "teaching" as philosophers such as Peters & Scheffler do, one finds a further interesting anomaly. The basic primitive, root concepts contain normative/ethical aspects and they imply a theory of action. In effect, I would argue that this educational theory is not just a scientific theory but a blend of ethical & scientific thought. This argument is long and involved and one I don't really understand but one I find appealing. My acquaintance with it arose in debates with more philosophically oriented educators in New Zealand; it stems from the philosophical controversies in England and U.K. among O'Connor, Hirst and Struthers as to whether educational theory is mostly scientific theory, mostly ethical theory, or a blend of the two.

In summary, my reconsideration of the Kensington analysis would involve me in what might be called a meta theory, the context of the theoretical interpretation. I think the data remain significant. I think we made a reasonable start in the theoretical interpretation. I'm struck with similarities in the findings of many other studies. I think the next step is toward a more general, codified and potent theory of education. I think such a theory will be a blend of science and ethics. I think it will be a theory of the practical and a theory of action. All this I find to be a large and interesting agenda. (Smith, 1977)

Summary

The intent of this study is the development of basic knowledge of the middle to longer term impact of innovation in American education. Most of the recent literature on innovation focuses on the immediate problems and processes of school change and does not attend to the longer term consequences (e.g., CERI 1973, Fullan 1972, Sarason 1971 and 1972, Rogers and Shoemaker 1971, House 1974, March and Simon 1958). Perhaps the major exception to this generalization is the SAFARI project (MacDonald and Walker, 1974) under way at the Center for Applied Research in Education at the University of East Anglia in England. SAFARI, Success and Failure and Recent Innovations, is a complicated out growth of the Humanities Project Evaluation and the two Cambridge Conferences on Educational Evaluation (MacDonald and Parlett, 1973 & Adelman et al 1976).

Over a dozen years ago we carefully observed, described, analyzed and reported, in Anatomy of Educational Innovation (Smith and Keith 1971), the first year in the life of the Kensington Elementary School in the Milford School District. As reported, Kensington represented a major attempt by a local school district to develop an innovative elementary school program. An open space building was designed and built. Staff was recruited from all over the Midwest. A comprehensive change strategy, what we came to call "the alternative of grandeur", was devised and implemented. A month long faculty summer workshop, including a week of T grouping, was instituted to provide organizational socialization. A variety of specific innovations - democratic interpersonal relationships (pupil-teacher, teacher-principal), individualized curriculum and instruction (no textbooks) and team teaching were interrelated. The present project, by one of the original investigators, proposes a major follow up:

1. locate, observe and interview the two dozen key administrators and teachers who originated the school.
2. return to the school and by means of ethnographic/participant observation procedures observe, describe and analyze the present workings of the school.
3. by means of historical data and interviewing of cohorts who have been at the school at different points in time develop a chronicle of the changes and an interpretation of the dynamics and process of organizational change.

In brief the hope would be to capture each part of the school and its original faculty at a second period in time, to make comparisons, and to draw inferences about innovation and its effects on the lives of a small group of people. Informal contacts with the individuals who were part of the school suggest that for some of the original participants the year or two at the school was a peak experience. Other casual data from the school suggests that Kensington as an innovative school passed out of existence and reverted to the "old Milford."

5. Design, Methodology and Procedures

Overview

A research project with multiple interrelated components and nested conceptual objectives poses a series of methodological and procedural issues. The parts of the investigation interlock in several ways. Briefly one interplay of time lines and structures would occur this way over two and a half years. First semester (1/78-5/78) begin the locating of people, address the problems of rights of human subjects, train the two assistants, refine the foreshadowed problems, build mutuality in district relationships, initiate trials of observation, interviews, and document seeking. Academic Year I (9/78-5/79) carry out the major portions of the systematic interviewing of former faculty, the full year of participant observation of the school, and the historical study. Begin the initial parts of the analysis. Academic Year II (9/79-5/80) major analytic effort and the writing of final reports. Also, return to the school and to further interviews of former faculty to refine interpretations. The write-up conceivably would involve several book length monographs and several shorter pieces directed to different audiences.

More specifically, the participant observer study of the present day Kensington would follow upon a long tradition of our earlier work. For brevity we would refer the reader to our previous accounts (Smith and Geoffrey 1968, Smith and Keith 1971, Smith and Pohland 1974, 1976, Smith and Schumacher 1972, Smith 1977). Over the years this work has been guided by the work of a number of investigators (e.g., Becker 1958, 1970, Beittel 1973, Bruyn 1966, Diesing 1971, Denzin 1970, Glaser and Strauss 1967, Homans 1950, 1967, Malinowski 1922, McCall and Simmons 1969, Scott 1965, VanVelson 1967, Zetterberg 1965, and Ziman 1968. Suffice it to say we would be around the school and the district, observing, talking, taking notes, collecting and reading documents, and trying to chronicle and to understand what is happening, how things are different, and how they got to be that way. In a footnote from the report of the original study the intensity of involvement was recorded this way:

During the study, school was in session 177 days from September to June. The workshop had involved four weeks in August. The observers have field notes from 153 different days at the school or in the district and 247 total entries. The latter indicates the overlap when both of us were in the field. Although it is possible to speak of 247 man-days of observation, this is faulty in the sense that some of the entries reflect part days and some reflect early morning to midnight days. One of our colleagues phrased it colloquially but cogently when he commented, "You were all over that school." The intensity of involvement is a key issue in the validity of the data. (Smith and Keith 1971, p. 10)

Our intentions are comparable for the proposed research.

The procedures regarding the follow-up study represent a major extension and elaboration of our case study methods. Several models are available (e.g., Terman and Oden 1947, Sears 1977, Skeels 1966, Nesselrode and Reese 1973). Specifically, several problems loom up as large. The first is locating

the people. While this will be harder than supposed, the investigator has been in contact with several over the years, several have taken Ph.D.'s and are in colleges and universities, some have kept up with each other. By telephone, letter, and inquiries at old addresses attempts would be made to track them down. Skeels suggests three essential qualities in this, "flexibility, ingenuity, and tenacity" (p. 28). Assuming their interest, willingness, and informed consent, each would be visited for two to three days of conversation, interview, and observation. The format of the interview would be a blend of narrative, open-ended questions, and focused questions. The content would reflect the concepts, problems, and issues listed earlier. Models would be similar to Gross et al.'s (1957) eight hour interview of school superintendents, Merton et al.'s (1957) focused interview, and the procedures of oral history (Baum 1971 and Dexter 1970). As much as possible of this would be taped for later analysis (e.g., Easley 1974, Smith and Brock - in process). The observations would be of the individuals in their current organizational contexts. Their teaching, their administering, their current collegial relationships and their professional activities would be observed and noted. Further, copies of their professional writings would be collected. Finally, we would readminister two inventories of attitudes and beliefs, the Minnesota Teacher Attitude Inventory (Cook et al. 1951) and the Teacher Conceptions of the Educational Process (Wehling and Charters 1969) which were first given 15 years before. The rapport and personal relationships of the earlier period (e.g., the many accounts in Anatomy) was/is such that the faculty will be honest and open. The researcher and his role in the past were perceived as careful and honest.

Data Gathering

The intensive interviews/observation/documentation thrust of the data collection is an extension of our earlier uses of participant observation and of the multi method, multi situation, multi concept and triangulation rationale. In our recent case study of the science education program in the Alte School District (Smith, 1977), a set of procedures evolved which prestage the 2 or 3 day data collection sessions proposed here in Kensington Revisited. Several of the teachers and administrators in Alte were "old friends" from earlier professional experience in graduate school classes and joint projects. We tape recorded long entry interviews with each and later "mid way" interviews and finally "exit" interviews with some. These ran from one to three hours a piece. In the course of these conversations/interviews they dug out from their files hordes of documents - class schedules, curriculum guides, curriculum evaluation reports (by staff and outside agencies), committee reports, published papers by themselves and their colleagues, staff bulletins, newsletters, and so forth. The stacks of information were literally a foot high in some instances. Also, we "tagged along" to faculty meetings, to PTA meetings and we joined them at informal morning pre school coffee klatches and day to day lunches in the cafeteria. Finally, we rode buses and walked with them on field trips with the youngsters. The data which was generated was rich in quantity and quality. Much of it was immediately relevant to our foreshadowed problems in science education much of it was more a "spontaneous conviction" on their part "you might be interested in this", an indicator of their own definition and phrasing of the problems and what was important and relevant. Synthesizing and integrating such data into a story or narrative on the one hand and an analytical/theoretical interpretation on the other was an exciting and creative task.

In Kensington Revisited, our hope would be to extend the techniques over a several day period. Procedurally it seems a logical extension. Motivationally we've found most individuals fascinated and stimulated by the opportunity to think seriously and talk freely about the depth, breadth, and nuances of their educational point of view.

Data Analysis

The cognitive processes in data analysis of ethnographic research are not well codified. In various appendices, symposia, and articles (Smith 1967, 1974, 1977, Smith and Geoffrey 1968, Smith and Pohland 1976) we have tried to specify our procedures. Most recently, in the appendix to Alte we sketched this sequence of intellectual procedures.

One of the most frequently posed questions in ethnographic research is "the formula" for generating analytical/theoretical conceptions or interpretations. In thinking through aspects of the development of CSSE, that is, while we were suffering through the process per se, a number of items arose; some of which we have highlighted before.

1. Foreshadowed problems: Always we seem to have an agenda of questions from students, from personal concerns about teaching/learning, from reading, from our earlier work. A number of these are listed...
2. Immersion in concrete perceptual images: This is really a question of how much raw data does one have. The day in and day out involvement in the setting produces an ocean of images of the phenomenon, a wealth of particulars—people, situations, events, occasions, etc. The human condition in all of its varied, idiosyncratic, unusual, mundane, exotic aspects plays itself out before one's eyes.
3. The interpretive aside: Along the way, a variety of ideas, insights, interesting associations of ideas, events, people arise. We tend to jot these down into the notes (Obs - ...). Later they become key points in developing the analysis. They seem to "pop out" in the normal give and take of observing and talking with people in the setting. Often they have a free associational quality ... "reminds me of..." and sometimes they are simple perceptual comparisons or contrasts.
4. Conscious searching: Concomitantly with the almost unconsciously determined items of the interpretive asides (item #3), there is the omnipresent question, "What does it all mean?" This is a search for overall patterns, for broad themes which seem to break the phenomenon into large chunks or domains. This is an active searching for order. Sometimes, as with the historical emphasis in the CSSE case study it came early (from reading Toulmin in general and Westbury in particular). It seemed to "keep working for me," in the sense of methodologically guiding toward interesting data and substantively, in turning up interesting problems and perspectives. It finally became a major theme (section #2) of the report. Further, it left me with a bit of unresolved tension in the form of "Next time, or soon at least, I want to do a for real historical study." With a bit of luck that may be in the offing. That brings one back full circle to a foreshadowed problem (in this instance, a mix of problem and procedure) and means

that that study is already cracked open enough to have a beginning point of attack.

5. Similarities and differences: The essence of concept formulation, and somehow I'd never quite seen it back in the days when I was administering the Wechsler and WISC is, "How are they alike and how are they different?" As items appear in the perceptual images, as verbal comments are recorded, as situations appear, as events come and go, one asks a simple two-sided question--How are they alike and how are they different? The similar things are grouped and given a label that highlights their similarity. The different things are grouped, insofar as possible, and given labels. There always seems a large "miscellaneous" category of items which seem important but which don't fit anywhere. The "seem" is critical. There's always a hunch lurking behind the "seem" and given more data, more time, and more thought, the pieces find a place in relation to one another. Earlier we called this the "jigsaw puzzle analogy."
6. Concurrent outside activities: During any project a number of things seem to be happening concurrently in one's life. Single strand existences don't exist. During CSSE I was
 - 1) at a conference in Germany which pushed me to read Decker Walker's book--and Westbury's chapter and also involved me with a variety of people and points of view. Some were new, some were old, both as individuals and from their scholarly work.⁷
 - 2) already reading Toulmin and Goodfield who had been suggested to me years ago as an overview to the history of science.
 - 3) chance reading - a graduate student had been talking about Sheehy's Passages.
 - 4) GIE committee's evaluating assistant and associate professors and all the problems of "effective professing" of which "effective teaching" was one major element.
 - 5) AERA - symposium and conversations with old friends.
 - 6) Seminar at GIE, everyone doing a p.o. project or case study. Mine was CSSE. Free for all in these.
7. Project press: As mentioned elsewhere, the short time line on CSSE produced enormous pressure to move quickly, to begin intensive attempts at conceptualizing early to seek workable outlines. This produced a series of stresses, some of which were toward conceptualization and interpretation.
8. Formal Analysis and Writing: Finally, in the end of May, and all through June, reading field notes, the summary observations and interpretations, and the multiplicity of documents produced the intensive search for order and the final patterns presented in the writing.

Implicit in other parts of the report and the appendix, one can find other remarks which round out the biography of the project. Hopefully, the discussion in the present proposal conveys both the continuities with the prior research and the new "wrinkles" arising from the idiosyncratic aspects of the new problem.

⁷A conference report, Theory and Practice in Educational Research, was published by the Institut für Didaktik, University of Bielefeld in 1976.

Historical Methods and Follow Up Studies

One of the most intriguing aspects of the proposed research (to the investigator) is the historical dimension of the project. While all the specifics are not clearly in mind, the perspective represents the coming together of a number of elements and experiences that have been provoking, if not irritating, for a number of years. They will be listed briefly.

What are the commonalities of historical method in the study of "cases" and observational method in the study of contemporaneous cases? The blend is particularly keen in George Homans (1941, 1950, 1962) studies of medieval England and his synthesis of contemporary studies. Recently the present investigator has found White's (1963) comments on "the logic of historical narration" the most insightful analysis and rationale for what we have been calling the "descriptive narrative" in our qualitative observational studies.

What resolutions exist in the controversy over the nature of explanation in history and contemporary social science. Hempel's 1942 paper on "the function of general laws in history" seemed to crystallize and focus the argument. Dray (1957) and Scriven (1959, 1966, 1973) seemed to be the major antagonists. Hempel replied with vigor in his long 1965 essay "aspects of scientific explanation." The historical perspective can lead also to a fundamental reworking of one's approach to the very nature of knowledge. Reading Toulmin and Goodfield's triad of books on the history of science, Architecture of Matter, Fabric of the Heavens, and especially The Discovery of Time was both provocative and unsettling in terms of specific ideas and conceptions of social science. In the Preface to a later book Toulmin (1971) expressed it this way.

The central thesis of the present volume...can be summed up in a single, deeply held conviction: that, in science and philosophy alike, an exclusive preoccupation with logical systematicity has been destructive of both historical understanding and rational criticism. Men demonstrate their rationality, not by ordering their concepts and beliefs in tidy formal structures, but by their preparedness to respond to novel situations with open minds--acknowledging the shortcomings of their former procedures and moving beyond them. Here again the key notions are 'adaptation' and 'demand' rather than 'form' and 'validity'... The philosophical agenda proposed here sets aside all such assumptions in favor of patterns of analysis which are at once more historical, more empirical and more pragmatic.

(1971, pp VII and VIII)

His point of view is a large agenda, indeed. It leaves one feeling more than a bit presumptuous.

Our major direct experience with historical method in the Alte study, as mentioned earlier. In that report several summary statements seem relevant.

The historical perspective on the district's science education program has seemed particularly instructive, even if only partially developed. To see--in the 1890's--that most of the elements, structures, and processes

were present in embryonic or rudimentary form may be too obvious to everyone to be called an insightful perception or conclusion. However, the implications sprout in a dozen different ways. What was the direct and indirect influence of the report of the Committee of Ten (1893) on the Alte community and staff at the turn of the century when they began to think about the high school program? What was the impact of the bulletins from the State Departments of Education in the midwest and elsewhere, such as Organization and Administration of Junior and Senior High Schools (Lee, 1927)? Was Alte a recipient or was it among "a number of superintendents, principals, and teachers (who) rendered valuable assistance in the preparation of such statements of standards. Are national and state committees and their pronouncements more or less influential now than fifty or one hundred years ago?...

And.

People and organizations who want to change other people and organizations should know where the people and organizations have come from, how they are currently structured, and where they want to go. The zoning of parts of Alte into one and a half to three acre lots sixty years ago is a chronicled fact. The interpretations that this led to "executive city" or to the current upper middle class quality of the community and the emphasis on educational excellence is overly simple and open to question. The relevance of this though to policy groups such as NSF or NIE and to more local immediate "change agents" however, does seem very great. Similarly Alte's conception of the good teacher is phrased colloquially as the "strong teacher," a person with clear goals for children's learning, high demands, and imaginative ways to get there. Such a normative structure has been developing a long time, as far as could be ascertained, and is the product or resolution of a number of contending views. It, like many other positions, is not to be "given up" easily.

In more practical procedural terms, the Kensington problem has several unique twists. There is available an intensive ethnography plus the original field notes on the genesis of the school. The proposed research anticipates an intensive ethnography of the current structure and functioning of the school. In an important and novel sense, we would work forward from the earlier data and backward from the current data.

Mediating between the two points in time would be documents and interviews (oral histories) of staff who were there at different points in time. In the Alte District, without searching strenuously, we found a large number and variety of sources. Conversations with the Milford superintendent suggests comparable files and records are available.

Insert Figure 9 about here

Development of a roster of teachers who have taught in the district in the intervening periods would constitute one part of the chronicle. Interviewing a sample of them would extend the narrative and interpretation.

1. Texts, lab manuals, teaching materials
 2. Alte Professional Evaluation 1976
 3. Alte Enrollment Forecast 1976
 4. Alte Staff Handbook (Continuously Revised) 1977
 5. Alte Curriculum Guides 1958 - 1977
 - math
 - science
 - social studies
 6. Alte School News 1962 -
 - Morning News
 - Evening News
 7. Local newspaper
 8. Outdoor Education Documents (state and local) 1976 - 1977
 9. Surveys and Reports by Outside Consultants 1975 - 1977
 - IPI
 - Community Survey of Citizens
 10. Reprints of articles, news notes, etc. that friends thought I might be interested in. 1976 - 1977
 11. Histories of the Community and School District 1931, 1977
-

Figure 9. Major Documents Consulted in Alte
(Adapted from Smith 1977 p. 124)

Community Analysis and Restudy

Recently, in trying to think through the methodology of the follow up of a school, Kensington, we ran on to a large body of literature on communities which have been studied over time. They are merely listed here:

1. Middletown (Lynd and Lynd 1929, 1937)
2. Elmtown (Hollinghead 1949, 1975)
3. Tepoztlan (Redfield 1930, 1950, Lewis 1951, 1960, 1970)

Methodologists such as Campbell (1970) have begun to explore the problems of single vs multiple qualitative observers over two or more times.

The specific bearing of this research cluster on both the substance and the methodology of school change at Kensington is not known. Perhaps most cogent is the discovery of how these investigators handled the contextual temper of the times, the Zeitgeist. In our case, the early sixties were just before the ESEA legislation and before the "revolutions" of the late sixties. Conceptualizing this, building a time line of events, and interpreting Kensington in such a context requires analogs and models. In brief some of the studies are new to the investigator, some haven't been looked at in years. Each would be part of the final preparations early in the Spring of 1978.

Rights of Privacy

One of the major methodological issues facing all social scientists is the rights of human subjects. Little empirical data exists regarding such issues. In an important sense returning to the lives of the Kensington staff can be construed either as a major invasion of their privacy or as a positive opportunity to recapture a major earlier period in their professional lives. By keeping an eye open and an ear alerted we hope to pursue some of the issues in the ethics of research. For instance, anonymity is a major proximal goal toward privacy. In talking with the current superintendent of the Milford District,⁸ regarding the feasibility of the project as a whole, mention was made of the original teaching staff. He commented that one of the original teachers had stayed on at Kensington for a number of years. On one occasion he had talked with her about the origins of the school. He reported "You know, she never talked much about the early years of the school. I asked her who the teachers were (using the list of pseudonyms from the original report) she wouldn't tell me." Such a piece of data strongly suggests the importance of the development and maintenance of anonymity of the individuals in a field study. Here was a superintendent who knew the names of the original faculty from original records, who had read our final report with the coded names, and who did not know the individual connections. Granted that he could find out with a bit of effort, he was content to leave them and their experiences to their own privacy.

8

Field projects have a way of beginning before they officially begin.

The Pupils: An Exploratory Study

Several readers of the preliminary proposal (both in the Graduate Institute of Education and on NIE's panel) have raised questions about an analysis of the children. In general, we have down played this in the proposal, for several reasons. First and perhaps most basically because the original study became (even though only partly by intention) a study of the adult life of the school. That seemed reasonable then as a means of delimiting the scope of the study. In some ways it still does. The concern for the reasons for change in the school has suggested a generally broader scope of the follow up - Kensington vis-a-vis the district and the local community. This in turn suggests a greater concentration on the children. Methodologically the problems involved in finding a random or stratified sample of the children seems a project in itself. The kind of problems that remain intriguing are the point of view of children who had only the one year in the school (e.g. the 6th graders during 1964-65) on which the initial study was based. In addition, a sample of youngsters who had no experience with the 1964-65 year, that is, entered kindergarten in or after 1965-66, would also be of interest. The mixed groups in between would pose other problems. All of this is merely to acknowledge an awareness of the issues, to be on the "look out" for children who spent some time in the school, and to begin to tackle the methodological issues involved in such a study. The substantive findings will be quite limited and guarded.

Summary

The field study methodology proposed in this investigation is directly related to the problems under investigation. The original staff of the Kensington School and the Milford District will be followed up with interviews and observations. Explorations of their points of view and reactions to the earlier experience will be sought. A return will be made to the Kensington School for a "special kind of ethnography", an account of the present day structure and functioning of the school and an historical chronicle and interpretation sketching the changes over fifteen years.

In conclusion, the methodology illustrates a simple but potent general strategy of research, "build each new piece of research about two thirds on procedures and instruments which you have used before and know what they can do and one third on new procedures which you are trying out and can interrelate with the older ones."⁹ The central thrust remains participant observation, a mix of observations, conversations/interviews, and document collecting. The new variations extend the conversation/interviews into oral history and modified survey procedures and extend the documents collection and analysis into formal historical search procedures. Comments have been made on several of the critical extensions.

⁹This idea arose many year ago (1964) in discussions with Fred Strodbeck in a summer conference on Learning and the Instructional Process. It has remained with this investigator as a major practical guideline. It probably can be rationalized as another specification of construct validity (Cronbach & Meehl 1956) or multi methods, multi trait approaches (Campbell & Fiske 1969).

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6. Plan for Organization and Management

Context

Organizationally and managerally, the proposed project is quite simple and represents a mode we have utilized in a number of previous investigations. Colloquially it's "half of me, a couple of graduate assistants, and a part time secretary". Such an organizational structure capitalizes on several key elements

1. The graduate assistants typically combine project activities and dissertations usually by "chunking" or splitting off a phase of the project. This brings a blend of collegiality, high motivation and creativity
2. The secretary typically becomes the information center for particular details of "what's where" both of records and current events; s/he's responsible for converting notes and tapes into multiple copies of typescripts.
3. The small staff minimizes time and effort on coordination of large numbers of people in varied times and places and reduces the hierarchical dimension and consequent problems of alienation.
4. The consultants, who were active in the original report bring a blend of that experience and their current work in sociology and education. They've already contributed ideas and criticisms to the proposal. They will revisit the school and critique the analysis.
5. The above frees the principal investigator to remain an active field worker and a supervisor/teacher, roles he both enjoys and has found productive.

The Time Line

The plan for the organization and management of the research can be sketched briefly but reasonably clearly at this point. The project breaks naturally into three major periods

1. Spring semester - 1978
2. Fall & Spring - 1978-79
3. Fall & Spring 1979-80

The Spring '78 period will involve four essential tasks. Getting project staff picked and underway: the selection and socialization of research assistants. My hope is to pick two people on the basis of a "performance test", prior work with me in class, seminar, or independent study wherein they have carried out successfully a small piece of field work. Announcements will go out concurrently, but tentatively (since we won't know if the project is funded) this summer for fall class enrollment, so that new students can enter the pool of possibilities without bias.

Initial contacts with staff in the Kensington School and the Milford School District. Explanation of the project. Securing of informed consent. Discussion of issues of interest to the faculty that might be built into the project. Soliciting of reactions, criticisms, ideas for alteration and elaboration of tentative plans.

Initial interviews with 3-5 of the original staff will be undertaken, partly as pilot, to iron out any complications in the interview/observations/documents triangulation approach. Also the interviews will be used to generate addresses, and locations, and histories of other participants to check out the problems to be encountered in locating people.

Development in detail, toward an axiomatic format, and in writing, of two large domains of social science theory - anthropological theory of cultural change (e.g. as summarized in part by Hatch 1972 and Kuper 1973) and organizational environmental theory (e.g. Thompson 1965, Rushing & Zald 1976, Baldrige & Deal 1974). This would be an attempt to move toward the theoretical/methodological issues of falsification of theory in qualitative case study context. One possibility is to have each assistant develop each position. The investigator would pursue his contrasting social psychological/educational position.

Beginning of the observation of the Kensington School. By observing for 6-8 weeks during the end of the 77-78 school year, this will permit a "three year look" in fifteen months. The main observing would occur in '78-79. Brief observations in Fall of 79-80 would round out the "third year." For some time, we have been taken with the possibilities and potencies of such a strategy, but have not had the opportunity to implement it.

The school year 78-79 will be the major data collection period for all three segments of the project - the interviews with former members of the faculty, the ethnography of the current Kensington School, and the historical chronicle of the changes in the school. The interviews of former faculty will be done mainly by the principal investigator. The assistants will probably develop separate foci with one involved mainly in the current ethnography and the second in developing the historical, documentary and interviewing aspects of the transition years. The principal investigator will be involved in aspects of this as well.

The major activity of the school year 79-80 will be data analysis and writing of final reports. A return to interview some of the original faculty will be made, if key issues have arisen along the way and more data is needed to fill in categories of analysis. Brief visits will be made to capture the third year in the Kensington School. Each assistant, insofar as they have had separate major problems, will systematically cross check data and interpretations of other parts of the project. This overaccents the separateness but see the appendix to Smith and Pohland 1974 on issues in collaboration. In larger projects (e.g. CSSE), this can be a major issue. As indicated earlier, the reports will include at least two dissertations and probably several book length analyses. Major dissemination efforts will be initiated.

In conclusion, for a number of reasons of personal preferences regarding research activities, the principal investigator would plan to be involved in the day-to-day nitty gritty of all phases of data collection, analysis and write-up. In addition he would hope to have two assistants, one in each part of the project to increase coverage, to provide another view, to increase the theoretical depth, and to help with the innumerable problems along the way. Some part of each study would be "broken out" or "chunked" to become their dissertations. Over the years we have had good success with this (e.g., Kleine 1966, Pohland 1969, Schumacher 1974).

7. Facilities and Arrangements

The facilities and arrangements for carrying out the research are supportive in several ways. Washington University and its Graduate Institute of Education have been and continue to be outstanding places to teach, to learn, to inquire. Traditions of academic freedom and excellence are long standing and deeply held. While these comments are easy to make, and while they sound like truisms or cliches, they do have a reality and an importance. For instance, while talking with the director of the GIE about budgets, time lines, and things to be done in the final submission of the proposal, he listened and interacted more generally. In the course of this "items that needed to be discussed with the Milford superintendent" were raised, explored briefly, and clarified. Similarly, copies of the preliminary proposal were circulated to faculty colleagues and doctoral students. Within an hour, one colleague of twenty years standing had read it, had commented in the margins, and raised a half dozen points where illustrations, specification and extension of the arguments would enhance the clarity of the proposal. Comments from others followed shortly. They have been debated, revised and incorporated into the proposal. Other faculty members with special competencies in historical method, organizational analysis, personal development theories of innovation and change have commented both figuratively and literally. "Write me in as an unpaid consultant...it looks like fun... I'd like to hear and talk about it as it goes along." The faculty is a stimulating group of "helpful individualists." The graduate students are being socialized similarly.

Teaching loads are integrated with both academic programs and research programs. For instance, in conjunction with the proposed research the investigator will continue to offer a graduate course, "The Classroom as a Social System" (Ed 524) for experienced teachers, administrators, and beginning doctoral students. The major textual material include Homan's Human Group Beittel's Alternatives for Art Education Research and the investigator's Anatomy of Educational Innovation. They represent three quite different sets of problems, content, and modes of thought, but all within the participant observational tradition. Subsequent seminars build upon these experiences and utilize Bruyn's Human Perspective, Glaser and Strauss' Discovery of Grounded Theory and Zetterberg's Theory and Verification. Mean while each student pursues his own participant observer project and the instructor works on his current project. Finally independent study projects build on these and lead into dissertations. The important point here is collegial socialization and stimulation, moving research programs forward, and full time work on a half time project.

Similar possibilities exist across departmental lines. For example, recently Professors Meltzer in Psychology and Nord in Business with a committee from a half dozen schools and departments have developed a new Ph.D. program in Organizational Behavior. The investigator is a member of that committee.

Initial discussions are underway with the Superintendent and staff of the "Milford School District". The district is both interested and supportive. The tenor of this is caught best in the superintendent's comment, "I'd like to be with you when you walk through the building again for the first time", which arose in our discussions of changes in the building physical structure.

Later discussions were held on the preliminary proposal and on the general letter and the specific criticisms from NIE staff and readers, which stressed, among other aspects the dynamics of change between 64-65 and 78-80. To the investigator's concern about "both data e.g. access to records and theory e.g. role of central office in a theory of school change", the superintendent, who is trained in historical research, nodded his head in agreement with the points and indicated no problem. Further, he suggested several documents and people that might be especially useful. Later, he introduced the investigator to the current principal and secretary of Kensington. He has written a letter of support which is on file. We have agreed to maintain anonymity as in the original study.

Contacts have been made with several persons from the earlier study. They see the problem as important, are willing to participate, and think the others would be also. Letters are on file from them.

Both Pat Keith and Paul Kleine, my colleagues from the earlier study, have read early drafts of this proposal, have made critical suggestions regarding substance and method, and have agreed to serve as consultants to the project. Professor Keith holds a joint appointment in Sociology and Education at Iowa State University; Professor Kleine is Chairman of the Education Department at University of Wisconsin, Parkside.

In short, except for picking the assistants, and even here interest has been expressed by several, everything seems tantatively in place regarding facilities and arrangements.

8. Dissemination Plan

The plans for the dissemination of the research findings follow directly upon the nature of the research itself and upon the general style and experience of the principal investigator. Since the research program is toward basic research (NIE, 1977) the central audience will be scholars in related and relevant fields. However, since the project itself is an instance of practice oriented inquiry (Atkin 1973) and deals with issues arising out of field settings it should be of direct interest to teachers and administrators. The following plans flow from these assumptions.

First, interim reports and a final report will be written and circulated among several, only partially overlapping, invisible colleges, those interested primarily in educational organizations and innovations, those interested in curriculum and instruction, and those interested in educational evaluation.¹⁰

Second, papers will be presented to organizations such as AERA, APA, and AAA. Over the years, the investigator has regularly been on the program at AERA, his primary reference group, and more sporadically at APA and AAA (mostly through CAE). In addition, the investigator has participated in a number of conferences (see vita) which have been instructive for him and which have provided a forum for quite broad dissemination of ideas. Further, he has been invited to give colloquia at Universities across the country. In the last couple of years these have included University of Rochester, Michigan State University and Stanford University.

Third, the final reports and the symposium statements will be converted into books and journal articles and find their way into standard repositories - libraries. One of the thoughts which it is hoped would be implemented is the publication of a more popularized version of the findings in keeping with the style of the new journalism (Wolfe and Johnson 1973). Writing as well as Terkel in Working, or Shaehy in Passages or Wolfe in The Painted Word seems a worthy goal, even if the probabilities are rather low of doing it well.

The major dissemination to practitioners has been in the past and probably will be in the future through the investigator's teaching and consulting. Part of this argument appears in "Relating theory to practice in educational research: some personal notes" (Smith, 1976) and part in "An Aesthetic Education Workshop for Administrator's (Smith, 1974). Essentially, the argument regarding changes in practitioners involves, time, personal contact, personal theories of teaching and administering, and data embedded in concrete holistic case studies. The teaching regularly involves teachers and administrators in the metropolitan St. Louis area. The consulting has been

¹⁰ One of the most interesting personal/professional experiences of this investigator has been the almost non-overlapping pattern of citations, conferences and discussions of Anatomy, Complexities, and Rural Highlands in the groups interested in innovation, teaching, evaluation respectively.

as varied as with school architects, technical assistance persons, curriculum evaluators and school district administrators.

A final possibility, which has not been budgeted, would be a conference of researchers and practitioners patterned after the Monterey Conference, "Exploring Qualitative/Quantitative Research Methodologies in Education" which was sponsored by Far West Laboratory, NIE, and CAE. In an unpublished set of "after thoughts", a summary paragraph catches the flavor of some possible changes.

Where does all this come out? One alternative I'd argue for is a reconvening of the group-a Monterey II. I'd run it longer, a month to six weeks. I'd give it a paper writing (rather than paper reading) focus. At a maximum I'd have one formal session per day-a 2 p.m. til 4 p.m. this-is-what's on my mind presentation and discussion type seminar. And perhaps a formal constraint that over the six weeks each individual is urged to have at least one dyadic lunch or dinner with every other individual where they look for commonalities in their approaches to educational research, theory, and practice. The culmination would be a volume of essays interplaying each individual's past research and future directions as stimulated and critically influenced by his summer collegial discussions. At that point, we might not only know whether revolution was on, we might have given it a push-or may be even slowed it down. (Smith, 1976)

3. ETHNOGRAPHY AS AN INTELLECTUAL PROCESS

An Evolving Logic of Participant Observation, Educational Ethnography, and Other Case Studies

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INTRODUCTION

The aspiration of the author in this chapter is straightforward but wide ranging: to provide a context and logic for the discussion of the genre of research that is coming to be known by such varied labels as educational ethnography, participant observation, qualitative observation, case study, or field study. For the most part, I will use these terms as synonyms.

Such an aspiration is warranted for several reasons. First, the method seems very simple, and some methodologists perceive it this way. Biddle (1967) called it "the broadest and simplest methodology used in classroom studies." The researcher goes into an educational setting with a pencil and pad, makes a few observations, takes some notes, and writes a report, a dissertation, or a book. For some it seems almost this easy; others stumble about and have incredible difficulty. Second, a growing group of researchers, evaluators, and policymakers (e.g., NIE's Experimental Schools, NIE's School Capacity for Problem Solving, Nuffield Humanities Evaluation, OECD's Center for Educational Research and Innovation, and NSF's Case Studies in Science Education) have been urging inquiry and evaluation using these techniques. Third, several major methodologists have disparaged the use of such procedures: the two most significant statements are Scriven's (1967) classic paper on evaluation and Campbell and Stanley's (1963) classic chapter on experimental research in education. The former attacked "process studies" and "noncomparative evaluation." The latter took a moral stand on the design these authors call X-O, or the "one-shot case study."

The Campbell and Stanley (1963) position on the one-shot study is:

Much research in education today conforms to a design in which a single group is studied only once, subsequent to some agent or treatment presumed to cause change. Such studies might be diagrammed as follows:

XO

As has been pointed out (e.g., Boring, 1954; Strouffer, 1949) such studies have such a total absence of control as to be of almost no scientific value. The design is introduced here as a minimum reference point. Yet because of the considerable investment in such studies and the drawing of causal inferences from them, some comment is required. . . .

FREDERICK ERICKSON, Harvard University, and PAUL DIESING, State University of New York—Buffalo, were editorial consultants for this chapter, and special thanks are due to Paul Pohland and Lee Shulman.

In the case studies of Design 1, a carefully studied single instance is implicitly compared with other events casually observed and remembered. The inferences are based upon general expectations of what the data would have been had the "X" not occurred, etc. Such studies often involve tedious collection of specific detail, careful observation, testing, and the like, and in such instances involve the error of *misplaced precision*. How much more valuable the study would be if the one set of observations were reduced by half and the saved effort directed to the study in equal detail of an appropriate comparison instance. *It seems well-nigh unethical at the present to allow, as theses or dissertations in education, case studies of this nature* (i.e., involving a single group observed at one time only). (pp. 176-177; italics added)

Being faced with such an edict, and with accumulating contradictory personal research evidence, posed an interesting dilemma to which we responded in various ways. In part we quoted other authorities. George Homans (1962) for example, introduced his discussion of the strategy of industrial sociological research with the epigrammatic comment: "People who write about methodology often forget that it is a matter of strategy, not of morals. There are neither good nor bad methods, but only methods that are more or less effective under particular circumstances in reaching objectives on the way to a distant goal" (p. 257). And we referred to the more devastating style and argument of Howard Becker (1970) that methodology is "too important to be left to the methodologists," and to his data, a presentation of the minimal overlapping of the research methods of the chairmen of the American Sociological Association's section on methodology and the methods used by the winners of the most prestigious sociological research awards (pp. 3-7).

In addition, we have tried to confront the issue directly with students and colleagues at Washington University in St. Louis. At one point some years ago, the following question was part of a set of Ph.D. examinations there:

Gouldner's books, *Patterns of Industrial Bureaucracy* and *Wildcat Strike*, are classics in the eyes of some social scientists. The methodology seems to be what Campbell and Stanley have called the "X-O" or "one-shot case study" (attached is a quote from their discussion). They state explicitly, "It seems well-nigh unethical" to permit such research. Take a position on the apparent dilemma and indicate how you would reason through toward a solution. Illustrate with reference to the substance of Gouldner's monographs.

In an important sense, this chapter attempts to develop a broader position within education, social science, and philosophy and to join the debate in the educational research community. The goal is to isolate significant methodological issues which can be examined in some detail, both theoretically and empirically.

In a recent unpublished version of his Kurt Lewin Award address, Campbell (1974) backed off from his earlier position. In a brief paragraph he makes the following points regarding qualitative case studies: (1) "such studies regularly contradict the prior expectations of the authors" (p. 24), (2) [such studies] "are convincing and informative to skeptics like me to a degree which my simpleminded rejection doesn't allow for" (p. 24), (3) "such a study [is given] a probing and testing power which I had not allowed for" (p. 25). In a concern for the division of labor in most large-scale projects, with the resulting partial knowledge of the several specialists, he also comments: "A project anthropologist, sociologist, or historian, assigned the task of common sense acquaintance with the overall context, including the social interactions producing the measures, could often fill this gap" (p. 25). While at heart Campbell remains

a quantitative experimentalist and quasi experimentalist, his more general points are "Social knowing . . . is a precarious and presumptuous process" (p. 29), and "If we are to be truly scientific, we must reestablish this qualitative grounding of the quantitative in action research" (p. 30).

My point in raising the Campbell and Stanley position and the reactions to it has to do with the difficulties in any prescription of research methods and procedures. The implications are several. First, such prescriptions are changing and evolving standards, group norms, if you like, of research communities and subcommunities within education and social science which are also evolving and changing. As such they may have a kinship with moral prescriptions. There are now a number of commentaries on the sociology of knowledge and scientific communities (Crane, 1972; Hagstrom, 1965; Kuhn, 1970; Lakatos & Musgrove, 1970; Ziman, 1968). These changing communities are one meaning of the "evolving logic" stipulated in the title to this chapter.

A second implication of the illustration, and of the title of the essay, is pedagogical. I will draw heavily on my own and my colleagues' research and methodological reflections to illustrate many of the general issues and problems. In a sense this is a reflexive mode of presentation, an attempt to try out Becker's suggestion for a "natural history." In this way a concrete, integrated, and contextual statement explicating the general arguments will be provided. Thus I believe I am stating, in a particular setting, Toulmin's (1972) more general point:

This thesis can be summed up in a single, deeply held conviction: that in science and philosophy alike, an exclusive preoccupation with logical-systematicity has been destructive of both historical understanding and rational criticism. Men demonstrate their rationality, not by ordering their concepts and beliefs in tidy formal structures, but by their preparedness to respond to novel situations with open minds—acknowledging the shortcomings of their former procedures and moving beyond them. . . . The philosophical agenda proposed here sets aside all such assumptions in favor of patterns of analysis which are at once more historical, more empirical and more pragmatic. (pp. vii-viii)

The theory under consideration in this essay is a theory of methodology within education and social science. It is a theory constructed to help solve methodological problems in studying teachers, curricula, classrooms, and schools.

Such a methodological description and analysis should culminate in a provisional codification of criteria and procedures which indicates a little about "how to do an educational ethnography or a participant observer case study," and it should present to a judge of ethnographic research proposals, or a reader of participant observer project reports, an image of a provisional set of criteria for a "good observational case study" or a "good educational ethnography."

DOMAINS OF KNOWLEDGE

General Overview

The general educational research community has only recently discovered participant observational research. A corollary to this discovery is the lack of knowledge of the substantial body of research that has been carried out with this genre of methods. In some quarters the belief seems to be that there is little precedent for such work and that there have been minimal attempts to speak to the methodological issues underlying the inquiry. To rectify this impression, four tables of references are presented here. While they are not exhaustive, they

should destroy the belief regarding little precedent, and the citations are numerous enough to enable the reader to begin his own program of criticism or self-training. In rough fashion they have been grouped into four clusters. Table 1 includes general references: studies of non-Western culture, modern communities, formal organizations, and informal small groups. Mostly they have been carried out by anthropologists, sociologists, and political scientists. Table 2 presents studies of educational settings: school and community, school systems, elementary and secondary schools, classrooms, and curricula and special programs. Table 3 lists major, seminal, methodological statements from fields other than education, and Table 4 lists those from education, many of which would be described as educational evaluation.

The most obvious limitation of these tables occurs in the slighting of the traditional anthropological research. In addition, the huge case study literature in clinical and individual psychology has been omitted (e.g., Freud, Erickson, and Piaget). Finally, the related, large body of literature on comparative psychology and animal ethology has not been included. For an early statement see Scott (1950), and for a recent review see Miller (1977). In fact, these tables have a personal quality, representing the gradually accumulating collection of materials I have read and found stimulating. Nonetheless, inherent in them, and well beyond, are important empirical problems in the sociology and psychology of knowledge. Genealogies, communities, and individual perspectives of these research workers and their interrelationships deserve empirical attention, much as Crane (1972) has done for mathematicians and rural sociologists.

A Personal Story

Every research worker has an interesting story to tell on the evolution of his or her own work. I believe that more of these stories need to be told if we are to have a useful and potent theory of methodology. The accounts of recommended training programs, not to mention the uncollated statements in graduate school catalogs, often have ironical contrasts with personal histories as they are recounted in various forms (Becker, 1970; Homans, 1962; Homans & Bailey, 1959; Murchison, 1961; Skinner, 1956).

My own experience in participant observation of natural settings began in discussions with Laurance Iannaccone and W. W. Charters, Jr. From them I came to know Homans' book *The Human Group* (1950), and soon I was into the literature of the overlapping groups of social scientists in the Society of Fellows at Harvard and Warner's Yankee City group. From Iannaccone and Charters also I learned of Robert Merton's *Social Theory and Social Structure* (1957) and the case study work of a cluster of his students, Blau, Gouldner, Selznick, and Lipset. A major center of activity existed in Chicago: Blumer, Hughes, Becker, Bruyn, Geer, Goffman, Lortie, Strauss, and the Waxses. Harvard, Chicago, and Columbia traded some people back and forth over the years. Some of these individuals began studying educational settings and problems. Most notable were Kimball and his students, Iannaccone and Hill-Burnett and their students. At Stanford, Spindler was training such people as Wolcott, Singleton, and Warren and developing his highly regarded series of monographs on education and culture. Recently several of these strands have become institutionalized in the Council on Anthropology and Education of the American Anthropological Association and in Division G, Social Context of Education of the American Educational Research Association, as earlier some had flowed together in the founding of the Society of Applied Anthropology.

TABLE 1
Domains of Observational Case Studies: General

Non-Western Cultures	Modern Communities	Formal Organizations	Informal Groups
Firth (1957, 1959)	Arensberg and Kimball (1940)	Arensberg & MacGregor (1942)	Becker (1970)
Malinowski (1922, 1935)	Barker & Wright (1954)	Blau (1955)	Festinger, Riecken, & Schachter (1958/1964)
Mead (1930)	Bruyn (1963)	Goffman (1959, 1961)	Gump, Schoggen, & Redl (1957)
Radcliffe-Brown (1922/1948)	Kimball and Pearsall (1954)	Gouldner (1954a, 1954b)	Homans (1950)
	Warner & Lunt (1941)	Lipset (1950); Lipset, Trow, & Coleman (1962)	Humphrey (1975)
		Polsky (1962)	Liebow (1967)
		Redl & Wineman (1957)	Whyte (1955)
		Selznick (1949, 1952)	Whyte (1953)
		Yunker (1977)	

TABLE 2
Observational Studies of Educational Systems

School and Community	School Systems and Interorganizational Educational Systems	Schools	Classrooms	Curricula and Program Evaluation	Teaching Careers and Student Teaching
Henry (1963, 1966)	CERT (1973)	Atwood (1960)	Cazden, John, & Hymes (1972)	Applegate (1971)	Becker (1951)
Singleton (1967)	Lutz (1962)	Barker & Gump (1964)	Cicourel (1974)	Beittel (1972, 1973)	Eddy (1969)
Spindler (1963)	Pohland (1970)	Becker, Geer, Hughes, & Strauss (1961)	Delamont (1976)	Berlak, A., et al (1975)	Finch (1978)
Warren (1967)	Schumacher (1975)	Charters, Everhart, Jones, Packard, Pellegrin & Wacaster (1973)	Elliott & Adelman (1977)	Eisner (1975)	Iannaccone (1963)
Wax, Wax & Du Mont (1964)	Smith (1977b)	Cusick (1973)	Gump (1967)	Easley (1974)	Sarason, Davidson, & Blatt (1962)
Wolcott (1967)		Iannaccone (1958)	Henry (1957, 1966)	Hall & Thurnau (1975)	
		Jackson (1968)	Leacock (1969)	Hamilton (1977)	
		McPherson (1972)	Lipnick (1976)	Munro (1977)	
		Mercurio (1972)	Rice (1984)	Reid & Walker (1975)	
		Reynolds (1973)	Smith & Geoffrey (1968)	Russell (1969)	
		Rist (1973)	Tikunoff, Berliner, & Rist (1975)	Self (1971)	
		Sarason (1971, 1972)		Smith & Carpenter (1972)	
		Smith & Keith (1971)		Smith & Pohland (1974)	
		Walker (1932)		Smith & Schumacher (1972)	
		Wolcott (1977)		Soloman (1971)	
				Stake and Easley (1978)	
				Walker (1971)	
				Wolfson (1974)	
				Wolcott (1977)	

TABLE 3
Methodological Statements on Participant Observation Field Studies: General

Payne and Hopkins	Monograph and Books	Collections
Becker (1953)	Becker (1970)	Adams & Preiss (1960)
Becker & Geer (1957)	Bruyn (1966)	Casagrande (1960)
Denzin (1971)	Denzin (1970)	Epstein (1967)
Glaser & Strauss (1965)	Glaser & Strauss (1967)	Garfinkel (1967)
Gump & Kounin (1959-1960)	Junker (1950)	Habenstein (1970)
Kimball (1955)	Peko (1970)	Hammond (1964)
Kluckhohn (1940)	Powdermaker (1966)	Jacobs (1970)
Malinowski (1922)	Walker & Adelman (1975)	McCall & Simmons (1969)
Meehl (1971)	Wax (1971)	Naroll & Cohen (1970)
Merton (1947, 1957)		Vidich, Bensman, & Stein (1964)
Scott (1965)		
Van Velsen (1967)		
Vidick & Shapiro (1955)		
Whyte (1971)		

TABLE 4
Methodological Statements on Participant Observation: Education

Papers and Chapters	Monographs and Books	Collections
Adelman (1976)	Boltel (1973)	Adelman (n.d.)
Atkin (1973)	Lutz & Iannaccone (1969)	Hamilton, Jenkins, King, MacDonald, & Parlett (1977)
Burnett-Hill (1973)	Parlett & Hamilton (1972)	Pohland (1972)
Easley (1974)		Roberts & Akinsanya (1976)
Erickson (1973)		Tikunoff & Ward (1977)
Hamilton (1976)		
Lutz & Ramsey (1974)		
Magoon (1977)		
Sindell (1969)		
Smith (1967)		
Smith & Pohland (1976)		
Wilson (1977)		

My own background—a mixture of Minnesota dustbowl empiricism in the Psychology Department and a kind of clinical educational psychology in the College of Education's Psycho-educational Clinic—had acquainted me with *none* of the anthropological and sociological investigators but with a variety of researchers doing case studies of individuals. That line of case study work has intersected very little with the more sociological and anthropological case studies. At one point (Smith, 1972) I informally tried to check the references to Piaget's clinical method in the various methodological statements by participant observers. There were none. More recently, people like Gardner (1972, 1973) have been bridging those domains. Finally, and again personally, an interest in the arts and aesthetic education raised the possibility of some additional kinds of case studies. Once again the ubiquitous Becker was already there empirically ("The Dance Musician," 1966) and theoretically ("Art as Collective Action," 1974). Beittel's seminal case studies (1972, 1973) on producing art seem unknown to most educationists. Hamilton and his colleagues (Hamilton, Jenkins, King, MacDonald, & Parlett, 1977) have produced a reader in educational evaluation, *Beyond the Numbers Game*, whose saucy and irreverent continuity tells a part of the "illuminative evaluation" story—complete with a manifesto and a prediction of a paradigm shift in educational evaluation.

These personal socialization sequences and casual observations of evolving networks, invisible colleges, and gradual institutionalizations deserve more formal attention than the informal curiosity which can be satisfied by reading prefaces, footnotes, and references and engaging in casual conversations concerning struggles for an intellectual perspective, for a peer reference group, and for legitimacy.

In summary, these comments are small but important examples of the evolving nature of scientific ideology and practice. They make the points that moral principles ("It seems well-nigh unethical") in research are norms of communities and subgroups of scholars in the social sciences and education; and that deviancy from community standards has some parallels to deviancy in other groups (Becker, 1966; Festinger, Schachter, & Back, 1950; Schachter, 1953). They also suggest that methodological pluralism has some benefits as well as hazards. For this chapter they indicate the labyrinthian and somewhat tortuous route one investigator took in searching for a theoretical-methodological rationale to solve the problems he was confronting. Traveling that path has helped to focus many of the more general perspectives of this essay.

Reference to a half dozen of our observational studies will occur throughout this essay, and a brief introduction to these should facilitate the analysis. They can be grouped into three periods: initial forays, a CEMREL period, and a recent period. Substantively they reflect a research serial which might be called an evolving ethnography of schooling. Theoretically they represent a cluster of middle-range theories which might one day cumulate in a general theory of education.

Early on, the Office of Education, in its small contract program, supported three of our studies: *The Complexities of an Urban Classroom* (Smith & Geoffrey, 1968), *Anatomy of Educational Innovation* (Smith & Keith, 1971), and *Patterns of Student Teaching* (Connor & Smith, 1967b). *Complexities* was an attempt to look at how a middle-class teacher coped with a group of lower-class children in a sixth-and-seventh-grade classroom of an urban elementary school. Procedurally it was our first attempt to implement a qualitative participant observer methodology. Through a series of commonsense decisions and lucky

accidents we hit on the "inside/outside stance" of William Geoffrey, the teacher, insider, and true participant, and Smith, the outsider, nonparticipant, and observer. We developed a device which we called "interpretive asides," which called for the inclusion of insights in the observational record. We split our records into in situ "field notes" and out-of-setting "summary observations and interpretations." The latter we dictated into a portable stenorette while driving to and from the school, which involved a half hour of critical time each way. Leaving the site, the percepts and ideas would be popping in a thousand directions. Capturing these before they were lost seemed essential. Early the next morning, on the way in, the residue that remained could be commented on when one felt fresh and relaxed. Some of the results of the study which still seem important were concerns about the way the school year began; the teacher's rôle in that beginning; the development of the social structure of the classroom, especially the rôles played by individual children; the conceptualization of teacher as decision maker and actor; and the characteristics of children from an inner-city neighborhood as these presented themselves to a teacher whose responsibility was to help the children learn.

From this class of Geoffrey's we moved to Kensington and the study of the first year in the life of an innovative suburban elementary school. *Anatomy* became a study of issues in innovation and organizational structure and process, a story of a group of educators attempting to build and implement the new elementary education. The significant results were descriptions and analyses of organizational development, of formal doctrine, and of the alternative of grandeur as a strategy of innovation. Considerations included the key themes of open-space building design; democratic administration, team teaching, and individualized curriculum and instruction, major elements in the new elementary education.

William Connor and I, intrigued with the unusual pattern of student teaching at City Teacher's College, spent a semester following a dozen apprentices around. Their "two by two" apprenticeship program included two weeks in a kindergarten, two weeks in a first grade, two weeks in a second grade, and so on through the eighth grade. Methodologically we became more serious about interviewing and began to think about triangulation and what we later came to see as an elaboration of the Campbell and Fiske (1959) multitrait, multimethod approach to valid data. Substantively, a variety of issues arose—"the nine trials phenomenon," aspects of anxiety, an analogy to psychomotor skills, and a model of socialization into the teaching profession.

The CEMREL period reflected an early commitment by Wade Robinson, the president of the Regional Laboratory, to the possibilities of alternative modes of educational inquiry and their relationship to educational practice. I spent a decade there, mostly with half-time appointments and released time from Washington University. One study continued directly the earlier line of work. Pat Brock and I began and still have several unfinished drafts of *Teacher Plans and Classroom Interaction*. We wanted to attack issues in the intellectual life of classrooms, we wanted to continue and extend the micro analysis of classroom discourse, we wanted to critique the stance of various in-vogue systems of classroom observation—Flanders, B. O. Smith, Taba, Medley and Mitzel, and Bellack, we wanted to move toward quantification, and we wanted to remain with a processual rather than a structural analysis. Consequently we tape-recorded a full semester of her first-hour Science I class, we took field notes along the way, and we have her daily pre- and poststatements about plans and results. CEMREL published our major methodological statement "*Go Bug Go*": *Methodological Issues in Classroom Observation* (Smith & Brock, 1970).

However, most of the CEMREL work was a series of forays into formative and summative evaluation using qualitative observational procedures, sometimes independently and sometimes triangulated with experimental and survey procedures in a three-legged evaluation model. The initial example (and perhaps most important) of these was "Education, Technology, and the Rural Highlands" (Smith & Pohland, 1974), a study of a computer-assisted instruction program. A series of papers (Smith & Pohland, 1974, 1976) grew out of that work. Substantively we were into community analysis, interorganizational theory, and the wonders of technology—both its doctrine and its realities. Methodologically we came to terms with "standard" participant observation technique, a phenomenon which does not exist, in our view. We elaborated the multimethod, multitrait position. And we tried to reanalyze and synthesize the Glaser and Strauss (1967) grounded theory position with our own.

Pat Carpenter and I spent a year in the formative evaluation of a social exchange token economy program in an urban school. In *General Reinforcement Project: Qualitative Observation and Interpretation* (1972), we raised critical issues in the implicit value stand of a behavior modification position, the simplistic doctrine of token economies and the complex behavior of the teachers, and some similarities and differences with Kounin's (1970) classroom management position.

Finally we were involved in several studies related to the Aesthetic Education Program. Sally Schumacher and I spent a year on *Extended Pilot Trials: A Description, Analysis, and Evaluation of the Aesthetic Education Program* (Smith & Schumacher, 1972). Later I observed a weeklong workshop for administrators (Smith, 1974), began toying with a variation of Piagetian clinical method (Smith, 1975b) as an evaluation technique, and took the observational role of learner in *Mrs. Kaye's Drawing Class: Some Theoretical Thoughts on Curriculum, Teaching, and Learning* (1975a). The perspective of the pupil seems an unexploited stance.

Our major recent activity has been an involvement in Robert Stake and Jack Easley's *Case Studies in Science Education* (1978), supported by the National Science Foundation (NSF). We studied the Alte School District (Smith, 1977b) an older suburban, upper-middle-class school district with the reputation of having a good science program. Key issues arose in district history, in the politics of curriculum change, in the nature of the strong teacher (as well as the prima donna syndrome), and in the conceptualization of curriculum at the district level. Currently we have two studies planned and underway: "Improving Urban Education: Federal Policy in Action" and "Kensington Revisited: A 15-Year Follow-Up of an Innovative School and Its Faculty." The former is an attempt to describe and understand a major effort in knowledge development and utilization in urban education. The latter is a simple "What's happened?" inquiry at the innovative Kensington School, including an attempt to find the original faculty, who are now scattered about the country, and to inquire into their current views of educational innovation and change. Both of these investigations are being supported by the National Institute of Education.

In summary, outside the dominant educational psychological paradigm in educational research, a large body of research exists within the qualitative, ethnographic, participant observational genre. Its roots lie especially in anthropology and in several traditions within sociology. A brief overview of one educational research practitioner's use of these methods suggests its applicability to a broad array of problems within education—schools, classrooms, curriculum development, and evaluation.

COGNITIVE PROCESSES IN EDUCATIONAL ETHNOGRAPHY

A number of stories can be told regarding the intellectual processes in doing educational ethnography, participant observation, and other case studies. On several occasions symposia at APA, AERA, and AAA, and in appendices to books and technical reports, we have tried to speak to issues in the cognitive processes involved in qualitative observational research. These attempts have usually been reflexive, that is, they have grown out of our musings and reflections as we have tried to use the methods in particular projects. In an important sense they have been attempts to indicate the dynamics of ethnography by attending to the creative processes in learning from a field work project. In this section I will summarize these thoughts. Once again this is a personal statement; it should be read as a semiintegrated collection of hunches and hypotheses open to more careful tests by empirically oriented students of social science methodology. How one does that testing is an interesting commentary on one's assumptions, practices, and theory of methodology.

Preliminary Phases

Origins of Problems. The vagaries in the origins of our research problems are captured best in the title of a recent short paper, "Accidents, Serendipity, and Making the Commonplace Dramatic" (Smith, 1978). The general point is that the problems are all around; they pass by the investigator in varying guises and for the most part need only to be recognized for their possibilities. A brief example or two must suffice. William Geoffrey was an MA student in a summer school course entitled "The Classroom as a Social System." As one of the activities each student took the Minnesota Teacher Attitude Inventory (MTAI) as part of a discussion of teacher variables in the classroom system. As Geoffrey turned his paper in he commented that this was how he felt but it had little to do with the way he taught. Such needling provoked a conversation, led to an invitation to see what it's like in an urban classroom, and eventually developed into *Complexities*. While observing Geoffrey's class I met an apprentice who was in this "funny two by two" student teaching program which was so different from the "regular" program at Washington University. Two programs which had common goals but such different structures just had to be interesting, so Connor and I set out on *Patterns*. When Cohen and Shelby approached me to get involved studying the Kensington School, it looked like a beautiful opportunity to see the origins of an elementary school faculty peer group, a phenomenon that was a very important part of Geoffrey's life at the Washington School. *Anatomy* grew out of that.

It might be argued that the three illustrations suggest an absurd model for the origins of research studies. Be that as it may, that's the way it seems to happen. It might be argued also that this is an applied extension of Underwood's (1949) old notion of "I wonder what would happen type" origin of research problems.

The Intuitive Feel of the Problem. When funding agencies, colleagues at other universities, practicing professionals (superintendents, principals, teachers), or graduate students considering dissertations raise with me the possibility of "the study of X, Y or Z," there occurs an almost immediate perceptual reaction-evaluation that it is or is not a good problem. Strangely perhaps, almost as an animal sniffing the air in an unusual setting, it comes out silently to myself as "It smells like a good problem." I have the impression that I could, at a minimum, defend it, or at a maximum get truly excited by it and be willing to commit one or more years to working on it. Usually the perceptual reaction is accompanied by a feeling of "Why didn't I see that or think about it before?" I don't understand the dynamics of the reaction, but it happens. It seems functional.

Guiding Models and Images of an End in View. Usually very quickly also there comes to mind a particular piece of research which captures the essence of what seems implicit in the problem and which serves as a kind of guide for what might be done. *Guide* is perhaps too limited an image, for not only does it give direction to the intellectual work but it brings a kind of confidence to the task; it legitimates the activity: "If we can do it as well as Jones did, it will be a worthwhile contribution." Several brief illustrations come to mind. In *Complexities*, a "simple" image dominated our orientation: Do an educational case study that would fit the half dozen presented and analyzed in Homans's *Human Group*. The logic was simple—a teacher and a classroom are just another group, as was the Irish farm family, the street corner society, or the bank wiring group. Further, Homans provided methodological and procedural guidelines, a conception of social science theory, a view of explanation, and a set of concepts and hypotheses appropriate to a middle-range theory of groups (and possibly a basis for a more general and abstract theory of sociology). In *Alte* the model was McKinney and Westbury's (1975) attempt to consider a school district and its curriculum from a historical perspective. We were on the hunt for a way of looking at science education in the Alte schools. Currently we have begun a new study entitled "Improving Urban Education: Federal Policy in Action." As soon as the label was generated it immediately raised an association with a study I'd casually known from years ago, "Project Camelot." A search in the library found it unavailable, but its author, I. L. Horowitz, had co-authored another book with J. E. Katz, *Social Science and Public Policy in the United States* (1975). A quick skimming (one section dealt with "Project Camelot") provided an initial guiding model.

Foreshadowed Problems. One of Malinowski's major contributions to the logic of ethnography was his distinction between "foreshadowed problems" and "preconceived solutions." As he put it a half century ago:

Good training in theory, and acquaintance with its latest results, is not identical with being burdened with "preconceived ideas." If a man sets out on an expedition, determined to prove certain hypotheses, if he is incapable of changing his views constantly and casting them off ungrudgingly under the pressure of evidence, needless to say his work will be worthless. But the more problems he brings with him into the field, the more he is in the habit of molding his theories according to facts, and of seeing facts in their bearing upon theory, the better he is equipped for the work. Preconceived ideas are pernicious in any scientific work, but foreshadowed problems are the main endowment of a scientific thinker, and these problems are first revealed to the observer by his theoretical studies. (Malinowski, 1922, pp. 8-9)

In a sense, Malinowski's statement calls the investigator to an awareness of the key problems, issues, and debates in that corner of the intellectual world in which the setting and the problem lie. The foreshadowed problems represent initial and partial analyses of the problem, the tenor of thinking of people who are working in related and relevant areas, and provisional modes of thinking. By way of illustration, at one time we joked about what one needed to know before starting a theory-generating observational study. Since we had just finished an educational psychology text (Smith & Hudgins, 1964) this seemed an appropriate spot to begin. The advice became, "Go write an ed psych text, then you are ready for a classroom observational study!" In retrospect, the truth in that advice seems to be in the residual questions after one has tried to read and synthesize several hundred research references (not to mention those that didn't make the bibliography). In the best sense these were Malinowski's foreshadowed problems.

Equally important for me has been the experience of teaching in an education department. My students have been undergraduates just moving into the teacher education program and experienced teachers in the M.A. and Ph.D. program in elementary and secondary education, administration, psychology, and guidance. As they raise their most perplexing questions, I store them away to ask the data from Geoffrey's class, from the Kensington School, from the Alte District, or wherever else we have been or are contemplating entering. In working with students as they try to learn the methodology of educational ethnography, a major problem arises if they do not have a wide background of problems generated by attempts to puzzle through large amounts of related but difficult-to-integrate literature, by an array of personal experiences or by difficult questions posed by colleagues. Dealing with that is one of the more intractable problems in teaching and learning participant observation.

Competing Theories. Many students and critics have found the conception of foreshadowed problems not adequate to their concerns for selectivity of data, seeing what one wants to see, and implicit theoretical biases. One of our further procedures has been to describe the tenor of the theoretical concerns that we have "gone in with." The best illustration of this comes from *Complexities*. In our educational psychology text we drew heavily on such theorists as McClelland, Homans, and Skinner. A more precise conceptualization of that position can be seen in the table of contents of that book, and contrasts and similarities can be seen in the table of contents of *Complexities* and, perhaps even more significantly, *Anatomy*. While elements of continuity exist, the real world of the classroom and school pulled us some distance from our initial stance. This acquaintance with prior theory reflects not only the early position of Malinowski but a similar more recent statement by Diesing: "The prospective field worker will acquaint himself with a variety of theories (the more the better) that may be applicable to his case" (1971, p. 142). Now, in long retrospect, the point of emphasis is slightly different. I believe we were implicitly running several alternative general theories against each other, that is, putting them in competition. In a sense we were unwittingly initiating an ethnographic paradigm for falsification. In addition, we were not only selecting from the several stances but also moving toward the beginnings of our own position.

Discussions with colleagues (Noian, 1975) suggest a further illustration. In a project involving the genesis of a community college, one alternative would be to enter this deliberately from the point of view of a sociologist such as Parsons and to look to the resolution of the functional problems and pattern variables and further clarification and development of that point of view. In keeping with the principle of competing theories, one might become as well versed in Homans's brand of interaction theory, Merton's functionalism, and March and Simon's organizational theory. As the events play themselves out in the natural setting, particular hypotheses within one position or another will be found to be less tenable than others. These beginnings of falsification, in the context of competing theories, seem to be the latent logical thrust of the Malinowski, Diesing, and Becker rules of thumb. Stated alternatively, approaching a setting with several competing theories, to each of which one is only partially committed, allows one to explore more fully the conceptual realities of the events in the setting. As events occur which the several theories omit, neglect, or speak to only minimally, the generation of one's own position comes to the forefront.

Thinking during Data Collection

While we are in the field observing directly, informally talking with and listening to participants, collecting and reading documents, a variety of discriminable intellectual operations seems to be occurring.

Immersion in Concrete Perceptual Images. One of the exciting and often unexpected events for a novice field worker comes from his/her immersion in concrete perceptual images. The day-in, day-out involvement in the setting produces an ocean of images of the phenomenon, a wealth of particulars—people, situations, events, occasions, and so on. The human condition, in all of its varied, idiosyncratic, unusual, mundane, exotic aspects, plays itself out before one's eyes. The potency of this overwhelming flood of unorganized data to disturb one's cognitive map of structures, hypotheses, and point of view cannot be overestimated. One sits in wide-eyed and "innocent" wonder and tries to capture, as much as possible, in the field notes and the summary observations and interpretations the drama going on.

The Interpretive Aside. Along the way, a variety of ideas, insights, and interesting-associations of ideas, events, and people arise. We tend to jot these down into the notes as "(Obs- ...)". They seem to "pop out" in the normal give-and-take of observing and talking with people in the setting. Often they have a free-associational quality ("reminds me of...") and sometimes they are simple perceptual comparisons or contrasts. This technique or procedure we sort of "fell into" while beginning *Complexities*. It seemed sensible to make at least a quick note of the "insights" or "bright ideas" that seemed to be arising effortlessly along the way. My hunch is that many get lost if not jotted down at the time. Later we found these to be very useful in that part of the analysis process we have called "generating concepts and hypotheses." Students whose notebooks are full of these seem to move the analysis along much easier than those whose notes are more limited.

Conscious Searching. Concomitantly with the almost unconsciously determined items of the interpretive asides there is the omnipresent question, "What does it all mean?" This is a search for overall patterns, for broad themes which seem to break the phenomenon into large chunks or domains. This is an active searching for order. Sometimes, as with the historical emphasis in the case study of science education in the Alte District, it came early, from reading Toulmin in general and Westbury in particular. It seemed to "keep working for me" in the sense of methodologically guiding me toward interesting data and, substantively, in turning up interesting problems and perspectives. It became a major theme of the final report. Further, it left me with a bit of unresolved tension: "Next time, or soon at least, I want to do a *for real* historical study." That brings us back full circle to a foreshadowed problem (in this instance, a mix of problem and procedure) and means that the next study is already cracked open enough to have a beginning point of attack. Such events remind me of Henry Murray's concept of serial, a longer-term unit in a theory of personality. He illustrates with friendships, marriages, and careers. More recently Beittel (1973) has adapted the concept for studying creativity in drawing. His argument is that understanding the production of any one piece of art requires knowledge of prior work and intended future work, an artistic serial. The parallels to cumulative research seem obvious. The conscious searching for patterns, in effect, is not only within the single project but also throughout the series of projects over time.

The conscious search for analytical or interpretive meaning moves concurrently with data collection. Glaser and Strauss (1967) have called the process "theoretical sampling": "data collection for the purpose of generating theory whereby the analyst jointly collects, codes, and analyzes his data and decides

what data to collect next and where to find them, in order to develop his theory as it emerges" (p. 45). A number of concepts are subsumed under the generic concept of theoretical sampling. Among these are saturation, slices of data, and depth. By saturation, Glaser and Strauss mean that no additional data are found which contribute to the properties of the categories under consideration. This is a useful but tricky concept. It assumes that one knows in advance or along the way what the key categories are and where the locus of information is. We have not found it quite that easy. As we work in a particular context or setting we try to exploit that setting for all of the information and all of the ideas we can find. In a sense, we keep looking until we can generate no more "insights" and "interpretive asides." It is at that point that we tend to quit. In this situation our experience has been that beyond the initial focus, the narrative or story line soon carries us into a whole variety of other problems and issues that we had not anticipated in our preliminary entrance to the problem. This is moving from the foreshadowed problems into the theoretical issues. The "Rural Highland" project was a beautiful example of this. Initially we had conceived the project in terms of the psychology of mathematics teaching. As we began carrying out our observation, however, the exigencies of the situation shifted the focus of investigation to the politics of education, interorganizational issues, the problems of introducing sophisticated 20th-century technology into an underdeveloped region, and the like. In a very real sense, the twin concepts of saturation and flexibility run parallel courses in the field work.

As is becoming explicit in the several items in this discussion of cognitive processes, data analysis occurs throughout the project, and also the social context of the project impinges on the intellectual aspects of the work. In our reflections on the *Alie* case study (Smith, 1977b), we raised the idea of "project press": "the short time line on CSSE produced enormous pressure to move quickly, to begin intensive attempts at conceptualizing early, to seek workable outlines. This produced a series of stresses, some of which were toward conceptualization and interpretation" (p. 128). We were on a one-semester, portal-to-portal time line: enter in January, final report due the first of June. We were finished (in several senses) on the first of July. Some of our colleagues in the other case studies were on shorter time lines—a month's observation and interviewing, with varying deadline dates. In every project we have been on, deadlines were posed by aspects of the projects themselves, by contracts for final reports, by AERA presentations, by new activities, by recurring responsibilities at the Graduate Institute of Education. These have exerted a press to finish particular pieces of work, and variations on the form of the intellectual processes of data collection, analysis, and write-up have developed. These were felt most intensely while still in the setting.

In one form or another these cognitive activities, along the way, appear in accounts of most field workers. They are vivid and potent experiences which contrast dramatically with images created by Campbell and Stanley's brief account of the one-shot case study. They seem more akin to an extended and sometimes multiple time series quasi experiment.

Final Analysis and Writing

The overall image I'm trying to arouse in this section on cognitive processes is intended to be one of creative thinking, the generation and construction of concepts, perspectives, and theories from an initial set of problems, through a long period of sought and unsought percepts and experiences in the field setting, to some final kind of order which appears as a written report. Its open-ended quality frightens some novices and critics and exhilarates others.

The Case as an Instance of a Class of Events. Eventually, if not early on, comes one of the most difficult and elusive problems, locating the case as an instance of a more general class of events. I think this gives researchers from other traditions great difficulty with the early stages of participant observer research, particularly as done by neophytes (e.g., doctoral students), because they cannot specify their problems clearly enough. The outsider seldom is content with "a description and analysis of X," for X is usually a particular concrete setting (e.g., Geoffrey's class, the first year of the Kensington School, science education in the ALTE School District). In a hypothesis-testing sense, there is no problem. The difficulty that neither the student nor the critic perceives is the theoretical complexity in coming to grips with what is the substantive problem in the investigation.

We tried to address the issue in the project entitled "Mrs. Kaye's Drawing Class" (Smith, 1975a). In coping with the "problem" under investigation, I constructed an abstraction ladder of one element, the kind of learning under consideration. At the most concrete level, I, as student-participant-observer, was learning to sketch wine bottles with charcoal. Sketching wine bottles is one instance of charcoal-drawing, which is an instance of one kind of representational drawing. Learning representational drawing is an instance of a broader class of learning artistic skills, which in turn is an instance of an even broader class, productive learning, wherein one produces something. In that study, I eventually decided that I wanted to work at the level of a theory of productive learning, interrelated with conceptions of curriculum and teaching. Alternatively, that research might have been conceptualized as a study in representational drawing or a study in artistic skills.

I believe this kind of process occurs throughout social science but that it is neither well recognized nor dealt with effectively in most observational research, or for that matter in other genres of educational research. Paul Meehl speaks to a similar problem in *Clinical vs. Statistical Prediction* (1954). Prediction, for Meehl, requires that an individual event be put into a class of events for which probabilities exist. Predictions of whether Jane Doe will go to a movie on Friday night will vary, depending on whether you know frequencies of Friday night movie attendance, of women attending Friday night movies, of women having dates, or of women with a recently broken leg attending Friday night movies. Jane Doe could be classified correctly into any one of those cells, but each would give a different probability. Other instances come to mind. Some years ago, in reading Skinner's *The Behavior of Organisms* (1938), one of the most brilliant and stunning, and, in hindsight, possibly misguided illustrations of this occurs in the first 60 pages, Chapters 1 and 2. For those interested in a model of theory building, his discussion of the experimental box as "environment," of white rat as "organism," of bar press as "operant," and of operant as "spontaneous behavior" is breathtaking.

For our purposes, Diesing (1971) captures best the importance of the issue under examination:

If I had to generalize at this premature stage, I would be inclined to point to the problem of the One and the Many as the essential problem of scientific method. Any scientific account of human society must somehow deal not only with the uniqueness of which human history and individual life histories consist, but also with the regularities of various sorts that appear in history. If I were to work out this problem in detail to determine how adequately various methods deal with it, case study methods would come out on top. They include both the particular and the universal within science instead of consigning the particular to intuition, practical application, or history; they exhibit the universal within the particular instead of segregating the two in one way or another; and they move from particular to universal and back by gradual steps rather than in one grand jump. (p. 296)

As we have indicated throughout this essay, such thoughts are continually salient. In the final analysis, if not before, the researcher must stake out the domain in what often seems like the shifting sands of multiple levels of abstraction across domains of theory.

**Skimming the Cream: An Initial Overview.* In the appendix to our study of the extended pilot trials of an aesthetic education program (Smith & Schumacher, 1972) we used a metaphor we called "skimming the cream." In a sense this is another perspective on what kind of a case one has. Here the procedure is more inductive and more "quick and dirty." In my view, metaphors from other occupations, life-styles, and eras probably do not carry the full meaning one intends, but they help. One of the consequences of research in a bureaucratic organization is being faced with deadlines. A second is limited time to do a task. The situation was this. During our last week of data collection we had to make a brief presentation of results to the several parties of the larger project we were investigating; these individuals were making decisions regarding the form the project would take in the succeeding years. The tactic we adopted was a simple one. In a local coffee shop, for a period of a couple of hours, we asked ourselves: "What are the major things we have learned from our year in the field?" As we brainstormed these ideas, with no reference to our file drawer of notes, interpretive asides, or summary interpretations (some of which were still untyped on tapes because of organization resource problems), we gradually accumulated a list of ideas, findings. We pushed and pulled on these until they gradually fell into reasonable broader topics and differentiated outlines. Points of debate were joined and countered with images recalled from classroom observations and informal conversations (interviews) with children, teachers, and supervisors. The most intriguing methodological question this raises is suggested by the "skimming the cream" metaphor. In this simple procedure, can the really significant rich items be obtained? Do the labored procedures suggested by Becker et al. (1961) in the *Boys in White* analysis and by Glaser and Strauss (1967) in their constant-comparative method yield more creative, more comprehensive, or more reliable theory and interpretations? Our guess is that the differences in creative propositions are probably minimal. Some comprehensiveness is probably lost by cream skimming. The reliability of interpretation, or perhaps better, the confidence in the interpretation, probably drops off more sharply. For students of methodology this is obviously a testable empirical problem.

Another, related aspect of cream skimming involves the preparation of initial statements of parts of the report. Here we have been caught with limited time to do the report—two months instead of a year. Essentially we have picked up on the brainstormed issues, returned to the notes to check them, elaborate them, and refine them. This is in marked contrast to alternative procedures of quick total review of all notes and careful page-by-page reading with cumulating analysis, or careful reading, categorizing, and tabulating as done in the constant-comparative approach. The empirical question remains. Does the quick and dirty cream skimming yield 20, 50, 70, or 90% of the total from the more systematic analysis?

**Developing Individual Sections.* From one perspective, the methods of data analysis I use seem terribly inefficient and unsystematic. After such processes of skimming our recollections for key items, and after the files have been organized into a chronology with separate sections for my field notes, my summary observation notes, the notes of colleagues and assistants, and the various kinds of documents, and after I have done some reviewing of the specific contents of the overall project, I usually start back at the beginning of the notes. I read along and seem to engage in two kinds of processes—comparing and contrasting, and looking for antecedents and consequences.

The essence of concept formation is—and sometimes it is quite seen it back in the days when I was administering the Wechsler Intelligence Scale—"How are they alike, and how are they different?" As items appear in the form of visual images, as verbal comments are recorded, as situations appear, as they come and go, one asks a simple two-sided question: How are they alike, and how are they different? The similar things are grouped and given a label that highlights their similarity. The different things are grouped, insofar as possible, and given labels. There always is a large "miscellaneous" category of items which seem important but which do not fit anywhere. The *seem* is critical. There is always a hunch lurking behind the *seem*, and, given more data, more time, and more thought, the pieces find a place in relation to one another. Earlier we called this the "jigsaw puzzle analogy" (Smith, 1967). This metaphor reflected not only the multiple pieces of a jigsaw one was trying to put together, but the very important aspect of actually shaping the individual pieces themselves. In time, these similarities and differences come to represent clusters of concepts, which then organize themselves into more abstract categories and eventually into hierarchical taxonomies.

Concurrently, a related but different process is occurring. Some time ago, I was impressed by Robert Merton's insight into social theory and social structure. The item that came to represent the totality was the beautiful label "latent and unanticipated dysfunctional consequences." My psychological background had urged a search for causes, for reasons, for determinants of a child's reading problem, for disaffection in school, for family difficulties. The conscious search for the consequences of social items, in all their combinations—latent and manifest, anticipated and unanticipated, functional and dysfunctional—seemed to flesh out a complex systemic view and a concern for process, the flow of events over time. In addition it seemed to argue for a more holistic, systemic, interdependent network of events at the concrete level and concepts and propositions at an abstract level. Zetterberg's argument for multiple ways of ordering propositions—inventories of determinants, inventories of consequences, chain patterns, and finally axiomatic formats—blended the theoretical with the concrete flow. At a practical level, while in the field, the thinking, searching, and note recording reflected not only a consciousness of similarities and differences but also an attempt to look for unexpected relationships, antecedents, and consequences within the flow of items.

These twin processes seem to capture the best of concept formation, in contrast to concept attainment, and of hypothesis development, in contrast to hypothesis testing. For us, they specify, almost operationally, the meaning of Glaser and Strauss's beautiful idea and label, "the discovery of grounded theory" and Diesing's equally apt term, "patterns of discovery."

There is a social dimension to these. No project occurs in isolation, at least when one studies social phenomena in one's own society. The importance of this for the gradually developing analysis and the progressive refocusing of data collection seems obvious to most field workers, and it is underestimated by most investigators using more set designs, whether experimental, quasi experimental, or survey. One illustration conveys the general point. As we were observing events at the Kensington School and talking casually with Professor Edwin Bridges, he took items about the fervor of the participants, the total time commitments, and the enthusiasm, and suggested that they sounded like Hoffer's (1951) true believers. Immediately the relevance of the more general conception was obvious. Later, Pat Keith found extensions to Klapp's (1969) crusades. Every project we have undertaken has always rippled in and out of other events in our professional lives. Such experience makes the "one-shot case study" label a serious misnomer. Everyday in the field is a new quasi experiment, guided and

enriched by an intellectually stimulating environment of persons, supportive and critical; of ideas, mundane and all encompassing; of chores and opportunities. These events play in and through the field experience.

Collapsing Outlines. In every participant observer study we have undertaken we have experienced a further phenomenon, one we have called "collapsing outlines." As we have begun analyzing the data, usually in terms of the foreshadowed problems which initially guided our entry and data collection, we have come upon interpretative asides and latent theoretical issues which seem a vital part of the setting and our understanding of it. As pieces are developed, we keep making tentative outlines that put some larger meaningful and logical order into the interpretation. Invariably the tentative outlines collapse in the face of more complex data and ideas. This seems another, later stage item in the definition and redefinition of the problems as a theoretical issue. Empirically, the dated sequences of outlines as we grappled with the meaning of our problem would be a helpful addition to this discussion.

Collapsing outlines seems very similar to a number of accounts of creativity in various artistic disciplines (Beittel, 1972; Gribble, 1970; Housman, 1933). While most artists begin a work with an initial idea, the gradually developing picture, poem or novel seems to develop something of a life of its own. Resolutions to particular problems create structures which not only constrain future decisions but also often suggest options which the individual creator had not perceived earlier. So it seems with problems, data, and analysis in participant observation. The initial problems are jarred by the interpretative asides. Recalcitrant pieces of data and negative instances are there and demand to be integrated. Finally, and most devastatingly, large, reasonably intact outlines tend to collapse because the weight of the data and the developing ideas in the analysis are too much for the earlier formulated conceptual structure. Eventually we have an outline which holds. It has a structure reflecting three major dimensions: integrity, complexity, and creativity. By integrity I mean it has a theme, a thesis, a point of view. The pieces fit together as an interrelated part-whole relationship. By complexity, I mean the outline has enough discriminable pieces to cover the major themes and the minor nuances, the large elements, and the nooks and crannies necessary to do justice to the system under study. Finally, by creativity, I mean the outline conveys some novel and important ideas to some relevant audience—the people in the system, the educational research community, and/or some practitioner who is teaching, administering, or working in the educational community.

Summary

As should be obvious by now, we have wrestled with and been guided by a number of field workers: Homans, Malinowski, Becker, Bruyn, Glaser and Strauss, Denzin, McCall and Simmons, Whyte, Iannaccone, Van Velsen, and others. All of them kept speaking to and answering questions we kept running into. Perhaps the most amazing experience has been rereading some of the early favorites (e.g., Homans and Whyte) and finding how much Homans and Whyte had learned and had to teach the second and third time through, several years later.

CLUSTERS OF MULTIPLE DIMENSIONS OF PARTICIPANT OBSERVATION

To this point I have argued that the educational research community exemplified by Campbell and Stanley has focused on the experiment and the quasi experiment as the dominant mode of knowing. I have suggested that such a view may represent the norms of the major community in education and psychology but that other social science communities—in anthropology and parts of sociology, especially—have developed their own traditions. Within education, a smaller subcommunity has evolved which has attracted a number of practitioners, researchers, and theorists and has begun to institutionalize itself more formally. More particularly I have described in some detail one researcher's attempt to implement, through a series of studies, this strand of participant observer research. Now, I intend to distance myself from those particular experiences and formulate a more abstract structure for analyzing and evaluating research work within this genre. This task seems both difficult and necessary. Regarding the difficulties, early on we thought that there might be a unitary phenomenon called "standard participant observation procedures" which could be used as a paradigm for analyzing and eventually judging any particular piece of educational ethnographic research. Paul Pohland and I (Smith & Pohland 1974, 1976) disabused ourselves of that hope. We argued then that on such dimensions as emphasis on the descriptive narrative, generation of theory, verification of theory, and quantification, important participant observer studies varied quite markedly. As a consequence, I am more inclined to ask for a clear statement of actual procedures and an intellectual cost/benefit analysis of those procedures for the problems at hand and the purposes in mind. In this section, four related clusters of concerns will be used to chart the multiple dimensions of participant observation.

While these clusters of concern pattern themselves like a piece of woven cloth, for analytical purposes they can be broken into warp, weft, colors, and textures. In order of increasing abstraction they include these levels: (1) data, (2) descriptive narrative, (3) analytical-theoretical-interpretive, and (4) metatheoretical. The necessity of such efforts arises in the needs of various individuals and groups who must make decisions in training students, in reviewing project proposals, and in judging research reports for publication.

The Data Level

In analyzing and judging a piece of observational research, a series of questions can be raised concerning the data and how they were collected. The resulting cluster of dimensions—direct on-site observation, freedom of access, intensity of observation, triangulation and multimethods, sampling, and attention to muted cues and unobtrusive signs—seems to comprise the major conditions assuring valid data.

Direct On-site Observation. At the simplest and most basic level, participant observation, in the sense of "being in" or "living in" the setting, involves the researcher directly in the social behavior under study. Being on site is the sine qua non of ethnographic research. It distinguishes the research from surveys, from interview studies, from laboratory studies, from testing studies. Such direct on-site observation assumes several conditions in social life. For instance, individuals in institutions, organizations, and groups often mask what is happening in the setting, for a number of reasons. That is, they create formal doctrines; develop facades, or perhaps "wallpaper over" significant issues and events. On such occasions any researcher faces major problems at the data level in regard to

what is "really" going on. This masking cannot be done so easily when one is observing directly at meetings, talking informally to participants at coffee klatsches, and taking part as people relate to one another in situ. While useful for many purposes, questionnaires, tests, and formal reports of events, insofar as they attest to social behavior and events, are indirect observations of those events and are susceptible to all kinds of distortions, both conscious and unconscious. The makers of K, L and F scales on the MMPI and other tests are wont to indicate this possibility when individuals fake good or fake bad, both consciously and unconsciously. In other settings and on other occasions individuals have been known to "stonewall," to lie, and to develop fictional reports to hide organizational realities.

The magnitude of importance of these points is so fundamental yet so "obvious" that it seems unnecessary to elaborate further. It emphasizes and reemphasizes our continual preoccupation with valid data of social events. In part our later discussion amounts to means to control or lessen possible difficulties in learning from such simple direct observation.

Freedom of Access. Conversations with observers—particularly short-term evaluators of educational programs—indicate a concern that they are being steered toward particular teachers, or classes, or schools or that they do not have access to other particular settings, people, or events. Usually these issues are settled at the time of entry; usually they are an interrelated function of the initial foreshadowed problems, the boundaries of the system under study, the more general purposes undergirding the research, and the evolving social relationships of the researcher in the setting. For instance, in *Complexities* the formal agreements that I had with the superintendent and the principal of the Washington School were that I would be in Geoffrey's class and go with him wherever his duties took him—lunch duty, yard duty, and so on. I was not to go into any other classroom unless invited. Our basic interest was his classroom, and that's where I spent most of my days. However, the clique of teachers with whom he associated included the eighth-grade "teacher in charge" and the clerk in the main office. Geoffrey set up a coffee bar where a number of teachers congregated and gossiped before school and at recess. We lunched usually with a group of the men teachers. I became good friends with several teachers and we chatted on several occasions through lunch and through their free period when the children were with the physical education teachers. While the focus was on Geoffrey's class, I was socialized more generally into the faculty of the Washington School. At Kensington, our agreements were such that all meetings—general, faculty, teams, PTA, and so on—were open to us, as well as all classes, curricular areas, and lessons. We were free to talk with anyone who was willing to talk to us. More recently in the Alte School District, in our study of science curriculum (Smith, 1977b), some of the staff had been in class with me at the university. We had discussed participant observer research in detail. When the project began they opened conversation with comments such as "You'll probably be interested in some of this..." and brought out stacks of curriculum guides, reports, newsletters, and so on. Once again, as part of the informed consent procedures, teachers willing to participate, after a discussion of the project, signed consent forms describing the kind of access we hoped for.

The main point: In studying schooling most field workers negotiate broad access relevant to the questions under study. Insofar as the relations among problem, purpose, settings, and events have been perceived initially, they are able to gather relevant data. A subcondition we try to emphasize is the freedom to

come to classes, meetings, and other events unannounced or without prior arrangements. Partly this represents convenience in maximizing use of time. Partly it broadens the basis of seeing normal or usual events and increases the validity of the data we are obtaining. When access is limited, for whatever reasons, questions arise regarding the adequacy of the data.

Intensity of Observation. Invariably one of the first and most critical questions regarding the validity of the data concerns the intensity of observation of the system under study. In general, field work is a labor-intensive research mode. In our own work on *The Complexities of an Urban Classroom*, I was there, "almost all day; every day for a semester," sitting in the back of the classroom, taking notes. Geoffrey, as teacher-researcher, was there all day everyday. In *Anatomy*, the intensity of involvement was recorded in a footnote to the study:

During the study, school was in session 177 days from September to June. The workshop had involved four weeks in August. The observers have field notes from 153 different days at the school or in the district and 247 total entries. The latter indicates the overlap when both of us were in the field. Although it is possible to speak of 247 man-days of observation, this is faulty in the sense that some of the entries reflect part days and some reflect early morning to mid-night days. One of our colleagues phrased it colloquially but cogently, when he commented, "You were all over that school." The intensity of involvement is a key issue in the validity of the data. (Smith & Keith, 1971, p. 10)

Several implications follow upon the issue of intensity of observation. First, the possibility of individuals "faking" their behavior, intentionally or unintentionally, seems less probable. The multiple actors are caught in a thick web of historical and contemporaneous interconnections. As observer, I kept listening and looking for offhand comments, raised eyebrows, hints that any one was behaving uncharacteristically. Reactions from pupils who had been in class the prior year, from teachers a grade below or a grade above, and from staff friends were constantly being scanned at the odd moment and setting of lunch or recess. Becker (1970) makes similar points in contrasting field work with the laboratory experiment. In the latter setting, the "subject" is totally removed from the social constraints of real life and is susceptible to experimenter intentions and nuances of experimenter behavior, as Orne (1962) and others have argued. In such a context the "pristine" qualities of laboratory data and results take on other shades of meaning.

The intensity of observation, in the sense of length of time in the field, interacts with the conceptual, theoretical stance one takes. In most of our work we have had a concern for processes over time, a theory of action. Early on, it was more implicit; more recently we have been trying to make it explicit. In regard to our data collection, we have tried to be around for a period of time that reflects commonsense boundaries—a semester, a year, the life of a project, and so forth. I suppose such units are comparable to the annual cycle in the life of a primitive group. Schools, in part, have an annual or semiannual opening or beginning; an establishment of order, structure, and routines; a long steady state period; and a closing or termination of the year. By observing throughout such a cycle one is privy to the special problems of phases in the social activities of the system, and one sees particular actors—administrators, teachers, pupils—coping with each other at critical, different points in time. Early on we had an intuitive feel that these data were more critical for teachers and administrators in our classes, in the sense of giving them help in thinking about their problems. Now we see this as a part of a larger theory of action conception.

The main point is that there is a need for a statement of the intensity of observation so that readers can assess the credibility of the results. A secondary point is a caution in moving toward a simple checklist-type judgment. The amount of time interacts with the scope and focus of the problem. A tertiary point is a common concern for adequacy of data across modalities of social science.

Qualitative and Quantitative Data: Traditionally, participant observers and educational ethnographers have shied away from quantitative data—from tests, questionnaires, or structured interviews. In my own case, early on I deliberately did so because I did not want to defend what we were doing in terms of the canons of the quantitative test literature. I had spent a year attending to the validity of children's personality and adjustment tests (Smith, 1958). I thought I was on to a very different kind of problem; I wanted it judged on another set of criteria. Now I think many of the arguments about qualitative and quantitative data are pseudo issues. Some field workers, such as Blau (1955), count kinds of interaction to make specific points. Others, such as Becker et al. (1961), quantify field note records regarding particular issues; on occasion we have taken issue with such procedures (e.g., Smith & Geoffrey, 1968, pp. 255–256, the two realities problem).

Some educational anthropologists (Cazden, John, & Hymes, 1972; Cicourel, Jennings, Jennings, Leiter, Mackay, Mehan, & Roth, 1974; Erickson, 1975), pursuing more specific substantive problems in the "new ethnography" rather than the broader, more holistic study of a group or a community, have moved to audio and video tapes of classroom events and the beginnings of quantitative analyses of these. From these records, they reconstruct the implicit meanings in speech and nonverbal behavior. My guess is that this tradition will gradually merge with the time-sampling tradition of study begun in child development by Goodenough (1928), Olson (1930), and others. In addition it will probably merge with the large literature on quantitative observation of teaching and classroom events, work well summarized in Dunkin and Biddle (1974) and Simon and Boyer (1967). A quick, nonquantitative survey of Dunkin and Biddle's book on teaching and Cicourel et al.'s (1974) book on language and school performance suggests a minimal overlap in reference citations. The work of Barker and his colleagues (1954, 1964) stresses direct observation of social settings and quantified records, but it too has remained as a nearly independent tradition. I find these to be puzzlements in the social science of knowledge.

Triangulation and Multimethods. Even though one tries to stay at the data level, the phenomena keep dragging one back to one's purposes and one's more general conceptions. Briefly I have indicated some of the dimensions of participant observation at the data level which enable one to analyze any piece of work and to begin to see the methodological structure of any project. A further argument needs to be entertained—that is, the nature of combining and synthesizing the multiple kinds of data implicitly and explicitly raised in the discussion.

Our attempts to deal with the issue have been mainly an outgrowth of Campbell and Fiske's (1959) multimethod, multitrait approach to construct validity in psychological measurement and Denzin's (1970) concept of triangulation. By triangulation, Denzin means the use of multiple kinds of data brought to bear on a single problem or issue. At an initial level, all of our studies combine direct observation interviews and conversations and document analysis, all of which we bring to bear on the issues at hand. A similar but more sophisticated analysis is made by Campbell and Fiske (1959). Their argument is simple and powerful. In checking the validity of psychological tests to give a picture of an individual, one needs multiple instruments dealing with multiple traits. The pattern of intercorrelations allows one to detect reliability coefficients and two kinds of validity coefficients: convergent coefficients, where the several instru-

ments measuring the same trait should be high, and divergent coefficients, where the test formats are similar but the traits are different and the coefficients should be low. At the time of their writing, and probably now as well, the psychometric data available on most instruments was severely limited.

In our work we think we are pursuing a similar paradigm, even though our data are qualitative and even though they are more a patchwork of partially filled cells and inferences. Figure 1 presents our elements of a multimethod, multiperson-

1. Methods
 - 1.1 Observation
 - 1.2 Informal interviews
 - 1.3 Documents: lesson materials, computer printouts, et cetera
2. Persons
 - 2.1 Pupils
 - 2.2 Cooperating teachers
 - 2.3 Principals
 - 2.4 Other teachers
 - 2.5 Multiple incumbents of multiple positions in multiple organizations
3. Situations
 - 3.1 Pupils at terminals
 - 3.2 Classroom teaching: announced and unannounced visits
 - 3.3 Multiple parts of the curriculum, in addition to arithmetic
 - 3.4 Multiple schools
 - 3.5 Multiple organizations
 - 3.6 Multiple parts of the country
4. Variables
 - 4.1 Individual: schemas, traits, motives
 - 4.2 Group: classroom interaction, activity, sentiments
 - 4.3 Organizational: schools, universities, R & D, Title III

Figure 1. Elements of a multimethod, multiperson, multisituation, and multivariable matrix. (Smith & Pohland, 1974).

son, multisituation, and multivariable matrix. The picture of the phenomenon—for instance, Geoffrey's classroom, the 'innovative Kensington School, the computer-assisted instruction in the rural highlands, or science education in Alte—involves the interrelation of data from different sources (observation, interviews, documents), different people (teachers, administrators, school board members), different situations (classrooms, schools, board meetings), and different variables and concepts—systems, norms, interactions, individual schemas.

When the items fit, agree, or are congruent, the picture evolves confidently. In the Alte High School, as I was trying to come to grips with the nature of social science, it was revealing that most of the faculty had degrees in history, that the psychology courses (and instructor) were not listed as part of the social science department, and that the social science department office sign said "History Department." Such items led to several interpretations about a dimension, degree of breadth, in social science at Alte High School. The central thrust of the multimethod approach seems to be an argument for internal consistency; the data hang together, the correlations would be high if data were scaled and quantified.

When the data do not hang together, one cannot throw out the tests or items and go back to the drawing boards. The problem is more difficult. Usually it sends one back for more data. Is an individual lying, seeing only part of the system, or ignorant of a whole set of events? Is the principal conveying his own wishful thinking of what science education is in his building? Is the faculty sketching out "what they are reaching for," "trying to do," rather than the day-to-day realities? Or is it, as the test makers might see it, that there is another trait or variable involved? In an important sense, one begins to reconstrue one's conception of the phenomenon. In a brief but powerful statement, Charters and

Jones (1973) caution about the risk of appraising nonevents in program evaluation. Specifically, they ask: "When has an innovation been adopted?" Their analysis produces a conceptualization of four levels of organizational activity: institutional commitment, structural context, role performance, and learning context. Such a conception puts order into the discrepant data one finds in formal documents and institutional plans, in discussion of administrative strategy and tactics, in teachers' day-to-day activities, and in pupil activities. The new conception sends one back for more data to check its adequacy. If the distinctions are sound, the data should diverge, just as correlations should be low on tests measuring traits which are independent (e.g., intelligence and weight).

Attempts to triangulate or to build multimethod matrices with qualitative data often result in congruencies which strengthen the validity of the picture one is drawing. When the data do not converge then one checks the points with more data, reconstructs the phenomenon, that is, makes more subtle distinctions than one began with, and then one goes for more data to recheck the new descriptive model, conceptual system, or interpretation. Van Velsen (1967), in discussing the final report and its credibility, argues for inclusion of lengthy excerpts from field notes. These notes, which include some context beyond the central point entered by the author, permit the reader to regroup and reorder them for her or his own analytical and verification purposes. Van Velsen's label for this is "situational analysis."

In short, these investigators are arguing for a common criterion—data from multiple methods brought together on common issues and presented in a way that the reader can perceive each source clearly and can begin to weigh the overall credibility and significance of the analytic interpretation.

The Sampling Problem at the Data Level. Whenever one cannot be everywhere in the system all of the time, one is faced with the sampling problem just as other social scientists doing more quantitative survey or experimental research are faced with the problem of sampling. Our comments here intertwine with earlier remarks (e.g., intensity of observation) and with later remarks on the descriptive and theoretical purposes of the particular projects. In the case study of science education in the Alte School District we were trying for a descriptive, analytic account of the nature of science education (math, natural science, and social science) in kindergarten through grade 12. Even for a small district, a hopelessly large task. With the problem stated—"What is the nature of science education in the district?"—a tentative sampling plan, Figure 2, was conceived.

SAMPLING DOMAINS

1. Principals
2. Schools and teachers
3. Classes and teachers by weeks:

	Cycle 1	Cycle 2	Cycle 3	Cycle 4
High School	Science	Social	Math	Alternative
Junior High	Team 1, 7th	Team 2, 8th	Team 2, 7th	high school
Elementary	School A	School B	School C	Team 1, 8th
				School D

4. Elementary Curriculum Committee: Science, Math, Social Studies
5. High School committees preparing for North Central Evaluation: Policy, Math, Science, Social Studies
6. Special circumstances:
 - a. Citizens, school board members
 - b. Knowledgeable professionals who have contacts in the district
 - c. Faculty meetings, PTA meetings, etc.
 - d. Special events, programs, activities

Figure 2. An early sampling plan for the Alte study (Adapted from *Science Education in the Alte Schools*, by L. M. Smith (NSF Case Studies in Science Education), 1977).

The logic of the plan was quite simple and was built on conventional wisdom about schools in general and Alte in particular. I talked with each of the principals. Typically, this was in the form of an open-ended oral-history interview: What's science education like in your building? This usually had several components: an hour of taped comments; a walk through the building with running comments on facilities, materials, and staff; an armful of documents; and initial meetings with particular teachers by chance and design. In effect they conveyed, at least initially, their perspective of science education—curricularly and organizationally, hopes and realities, problems and successes. It seemed that I needed to see teaching and learning in process; consequently I devised a possible way of cycling through schools, levels, and domains of science during the semester of field work. Early on I found there were curriculum committees and, even more importantly, by chance, that Alte High was in its self-study year, getting ready for a North Central Evaluation. I was permitted to and did join some of those 7:15 a.m. discussions. A number of special circumstances also arose. I interviewed people with special perspectives on the district—long-term residents, parents, school board members, professionals who enter the schools for various reasons (psychologists, university professors, etc.), and districtwide central office personnel. Some of these were early "Tell me about science education in Alte," and some were more exit interviews. The latter started with "Tell me" and soon evolved into a series of "How about . . . ?" wherein I raised interpretive hypotheses. The major point here, however, is that we had a rough initial image of the territory. We tried, through interviews, observation, and document analysis, to cover that territory.

Time restrictions in Alte caused us to modify in several ways the more formal plan outlined in Figure 2. For instance, the classroom observations shifted to an initial concentration on the junior high school—every science, math, and social studies teacher was observed at least once, and a number were observed several times. Usually these were intertwined with brief to several-hour interviews. Most of the elementary school observations were concentrated in two schools. At the high school I observed several teachers in each department—usually a mix of those I had known and not known. Typically these were of the unannounced "May I sit with you this morning?" type. No request was refused; often there were comments such as "Today's a lab . . . a review . . . a film" and so forth. I said "Fine" and went in to see what was happening. Usually I was given a copy of the text or lab materials, and a brief comment or two locating the lesson in the broader perspective of the course, department offerings, or grade-level sequences. Similarly, I hunted documents—high school yearbooks, curriculum guides, reports of curriculum committees, and, significantly, the *Alte School News*, a newsletter to the citizens of the community. Some of these I scanned, some I read intensely with particular questions in mind. Again they presented a sampling problem.

Most participant observers don't speak to the issues of sampling. In our view it lurks behind every decision the investigator makes when he elects to be here or there, to spend more time here rather than there, and decides what array of documents to read, of people to interview, of settings to hang around. At the data level, the question is always, "Have I seen the nooks and crannies of the system as well as the main arenas, to give a valid picture of the system?" The main criterion we strive to meet is to know the total system better than any participant, who is often restricted to a particular niche or position in the system. As we learn more about the system at its concrete level, we find areas we hadn't anticipated, and we find we have to shift plans. This shift in plans has been called "progressive refocusing," by Parlett and Hamilton (1972) and speaks to the flexibility of

the methodology. Similarly, Glaser and Strauss (1967) have spoken of "theoretical sampling." In their discussion, they are attending to the problems of the kinds of ideas and theory being generated and the shift in activities at the data level as the more abstract problems under investigation are clarified. My central point is that the participant observer, in a procedures chapter or a methodological appendix, needs to convey to the reader his initial plans and intentions, the changes along the way, and the final resolutions and accompanying reasons. In this manner, the reader, or the skeptic who wants to replicate, can see clearly how and why he did what he did.

Muted Cues and Unobtrusive Signs. Close observation in a setting produces what Andrew Halpin (1966) has called "muted cues" and what Webb, Campbell, Schwartz, and Sechrest (1966) have called "unobtrusive measures." When the clerk in the Washington School sat silently "working" within earshot while I had my entry conversation with the principal, and two weeks later she thought it would be a good joke on the principal to make out a central office timecard for me to sign first and then for him to sign, I began to form an image that she had more to do with the social structure of the school than the formal organization chart indicated. In spite of my initial anxiety, it turned out to be a good joke, she thought, and thereafter she cheerfully volunteered neighborhood and school stories and items "for your book": items from the mailman who walked through the community daily, from local shopkeepers, and from parents, not to mention items from downtown office personnel that came by her desk. In effect, the muted language, the unobtrusive traces of social life, suggest a host of important, often un verbalized issues that enhances the quality of data available to the directly observing inquirer, in contrast to the inquirer not in the setting. In project proposals, which are based on some pilot observations, I look for data reflecting muted cues and unobtrusive measures. Final reports which don't contain such data seem less significant than those that do.

Summary. In the analysis of the structure of participant observation at the data level, my contextualist bias has encroached to demand that other elements and the multiple interrelations among the elements be stressed. Careful analysis of the project's data does begin to locate it in a position from which judgments of quality can be initiated.

The Descriptive Narrative: A First-Level Interpretation

In considering the multiple dimensions of participant observation, we have been inquiring first into the adequacy of the data generated by the researcher, in the course of which we have commented necessarily but fragmentarily on the uses to which the data are put. Since first reading Homans's *The Human Group* (1950), I have been struck with the impact of his descriptive accounts, in everyday language, of a group, organization, or society under consideration, and with his attempts to conceptualize these events in more abstract concepts and propositions. Most of the groups we have studied have evolved over time, and we have usually wanted to grapple with the change processes in the group during these periods. The latent agenda seemed to be a belief that the process issues (in contrast to the structural) are more critical for teachers, school administrators, and curriculum developers, our usual audiences. This has led us to argue for "telling the story" of the characters, settings, incidents, and on occasion the drama of conflict, crisis, and denouement. More recently we have been persuaded by White's (1963) analysis of the logic of historical narrative that there is not "a" narrative or "the" narrative; rather, multiple narratives might be written.

White's Analysis. In his "The Logic of Historical Narration" (1963), White makes a half dozen points which clarify the nature and assumptions of the descriptive narrative in participant observation. He sets the stage with a definition of history:

... every history is a history of some entity which existed for a reasonable period of time. . . . the historian wishes to state what is literally true of it in a sense which distinguishes the historian from a teller of fiction or mendacious stories, and the task of the narrator is to give a connected account of the entities' development in time. (p. 4)

Participant observers, particularly those of us interested in processes of education, should have little difficulty with this initial statement.

Next White distinguishes "a chronicle" from "a history." The former is "a conjunction of non-explanatory empirical statements which expressly mention that subject and which report things that have been true of it at different times" (p. 5). A history is distinguished from the chronicle in that a history employs the notion of explanation. At this level the chronicle is close to our data level. His comments regarding facts in the narrow sense, statements about conditions and statements about events, follow upon this. For White a chronicle is true insofar as its component statements are true. Even as chronicles move toward historical narrative, he expressly indicates that this requires no necessary commitment to a covering law or regularity model of explanation. Nor, in his judgment, is it a commitment to explanation by causes.

Third, he embarks on a major discussion of the meaning of one history being better than another, when both are "true" in the sense of the truth of the items in the chronicle. He comments that historians have developed reasonably clear statements of alternative positions regarding this level of their histories. The list of labels includes subjectivism, essentialism, big battalia history, encyclopedianism, and scientism. Again they seem congruent with various stances on ethnography and participant observation.

Fourth, he argues that calling one history a superior or better history than another piece of work involves more than judgments of truth of facts and truth of causal statements. Finally, regarding historical memorability, he leaves the analyst with a relativistic and contextualist problem: "I know no rock or historical practice or usage upon which to rest some definition of historical memorability" (p. 26). In my judgment, that takes the participant observer, insofar as his descriptive narrative is like an historical narrative, back to a broader contextualist stance of his purposes, his priorities, his problems, and his situation, and the purposes, priorities, problems and situations of his multiple audiences. In a sense this is every researcher's concern. In another sense, I am arguing that the participant observer has a special and potentially very powerful stance toward these issues.

Beittel's Analysis: Presentational Modes. Explicating Kenneth Beittel's approach to participant observation constitutes an exercise in the totality of the methodology. However, his point of view regarding the several narratives which might be written is central to his analysis and provides a complement as well as supplement to White's historical analysis of narrative. In contrast to most participant observers, Beittel focuses on the individual rather than the group. The individual in this case is in an interesting setting, a basement laboratory in the art education building at Pennsylvania State University, and he is involved in "the making of art," a series of drawings using pencil, charcoal, and ink. The laboratory permits time-lapse photographs, notes on the process, and interviews stimulated by the photos. The two-hour weekly sessions continue for ten weeks.

Some individuals return for several semesters. Beittel's major assumptions are twofold: "... to study the making of art one must move as closely as possible ... to the creating stream of consciousness, and [secondly] ... a special participant observer role is essential to this closeness" (1973, p. 8). At the data level, Beittel has (1) mute evidence, that is, the pictures themselves; (2) iconic representations, videotapes and motion pictures of the artist in action; (3) process representations of the evolving art work; that is, the time-lapse still photographs of the developing picture; and (4) the notes kept by the observing researcher and recordings of the interviews between the observer and the artist.

From these data, Beittel argues that several kinds of narratives can be developed. Several of these remain very close to the data. The "first-person singular narrative" is an attempt to "reunite the available information on a drawing's evolution as though the artist were thinking to himself as he works" (1973, p. 34). His examples, such as this one of Larry's, graphically convey the portrayal: "Let's see ... I'd like to try to get that bad downtown Baltimore feeling. I can see it. How to begin? Like I'm right in the middle, everything stronger than me. But how to get it on the page? What line to hang it on? They need to fan out like. Guess I'll just jump in. Stronger, bolder strokes. There ... better watch how I mix black and white paints. I'll lose that grunginess of outdoors against that black, black inside feeling—the bar, the strip show. The black's gotta count ... " (1973, pp. 36–37). But as gripping as the narrative is, it loses for Beittel some of the broader context, both in the long-term serial perspective and in the host of additional elements of meaning and feeling about "downtown Baltimore."

Beittel tries next for what he calls a "multiple consciousness narrative." The artist, Beittel, an assistant, and a visiting psychologist all have taken part in the participant observation of the drawing process. The descriptive narrative produced by these multiple consciousnesses stays at the educated layman's level as the several individuals come to grips with the events and conditions of "a unique time-space artistic process."

Beittel labels a third kind of narrative "literary psychology." It represents an observer with a particular theory in mind which guides his perceiving and reporting but which still remains couched in the language of the general culture. Frequently used theories are those of Freud, Jung, Langer, and so forth. This mode of narration gives Beittel great difficulty. His problem, so it seems to me, is his own creativity. Consider, for example, the following comment:

My procedure, in actual practice, has been that of coining new terms and labels to aid my perception and description of the individual case, arriving at these neologisms as inductively as possible. Since, however, I use the new terms for more than one case, they function somewhat like principles from psychological theory. In truth, they function more usefully when I discuss the problem of representing individual cases in the abstract, or in the general. When I actually speak of a given individual, the terms do not occur as readily. In this book, for example, I have spoken of "artistic causality," "idiosyncratic meaning," and "intentional symbolization." (1973, p. 44–45).

If I read him correctly, Beittel has the beginnings of major sensitizing concepts which reflect both inner and outer perspectives and which have considerable power in thinking about artistic creativity. Informally we have tried to test this potency hypothesis by equating artistic creativity, teacher creativity, and researcher creativity and looking for analogues to Beittel's concepts in these other domains. But my major point is that the generation of these concepts plays back into his observing and narrating. As a consequence, the literary psychology narrative begins to take on a considerably more theoretical appearance.

In what he calls "historical and interpretive modes twice removed from the artist's stream of consciousness," Beittel distances himself in time and specific occasion to develop narratives which I am more inclined to call analytical or theoretical accounts. They continue to have a curious blend of the artist and the observer. In essence he tries to draw out the "artist's conceptualization about making art" as the artist reflects on what he's done. The analogue seems to be similar to our several attempts to capture a teacher's theory of teaching or a principal's theory of administration, that is, to determine what are the working concepts and propositions. Finally, in some if not most or all artists, Beittel pushes toward what the depth psychologists are prone to call more basic or fundamental levels of functioning, and what he calls "the artist's superordinate concepts on the making of art or idiosyncratic artistic myths." These are the more all-inclusive accounts of the meshing of an individual's life and his art. His major examples are autobiographical, "the Bach-music landscape theme" and "the river theme." In time they stretched over decades; in space they ranged from the Susquehanna River to the battlefields of France; and in meaning they captured the full reaches of an evolving life perspective.

Finally, he takes a major position on the observer's relation to the individual(s) he studies. This he labels the "formative hermeneutic mode," a stance which seems close to a Rogerian counseling relationship. Understanding and helping intertwine. The narrative takes on an action perspective.

This commitment to a narrative as one of the outcomes of participant observer research has further major implications on the kind of sociological and psychological theory that one generates and uses. I'm reminded of Becker's arguments in the preface to *Sociological Work* (1970), wherein he sees society as collective action, people doing things together, and sociology as a study of the forms of collective action. Any talk of structures and functions must come back eventually to individuals doing something together. Similarly, Homans's (1950) early admonitions about the "big" words of sociology—status, role, culture, authority—must eventually lead back to human beings doing things in particular times and places. In psychology, Henry Murray's (1938) concerns for persons, situations, actions, proceedings, and serials, not to mention needs and presses, reflect a point of view, a theory, which is compatible with results presented in narrative form. More recently, Sarbin (1977), in the *Nebraska Symposium on Motivation*, has argued that psychological theory needs to be reconstructed around a root metaphor or world view of contextualism which implies, for him, a dramaturgical perspective, units built on historical events and interacting individuals, and concepts such as scenes, plots, roles, and actors if one is to deal with creativity, novelty, and change in the human condition. By implication he is arguing that other theories are less amenable to these issues.

Summary. This discussion of the descriptive narrative has shown how complex and problematic is what we once thought was a necessary but simple task—telling the story of the case under investigation. By appealing to White's analysis of the historical narrative and Beittel's imaginative set of alternatives, we now see a fuller set of distinctions about the narrative. The implications for the kind of psychological and sociological theory are also apparent. The data problems, the metatheoretical dilemmas, and the theoretical stance all contribute a context to the narrative. The individual ethnographer in doing his work is faced with a series of contingent decisions. He who would judge a particular piece of ethnography faces a task no less complex. To think otherwise is to make a serious mistake.

The Theoretical-Analytical-Interpretive Level

In the discussion of clusters of dimensions which can be used for analyzing and evaluating participant observer research, the argument has proceeded from the concrete to the abstract. A number of concerns were raised regarding the quality of the data. At the next level of abstraction, it was argued, most participant observers want to "tell the story" of a group, an organization, a community. To some, such as Erickson (1975), Lévi-Strauss (1963), and Wolcott (1975a), this is the core definition of ethnography. Most researchers, however, want to move more abstractly into what is typically called theory, analysis, or interpretation. At this point, the field splits apart and almost all commonality is lost. For purposes of clarity I propose to describe briefly the evolving position we have taken in much of our work, and then to indicate alternative positions. The arguments for the pros and cons of those positions involve a series of metatheoretical dilemmas, controversies which have plagued social science and philosophy for a long time and which seem to have no clear solutions.

In recent years we have been urging our students to make explicit the theory that is implicit in their studies; that is, we have asked a student, Jones: "What is Jones' theory of . . . ?" Over the past few years this has yielded Wood's (1977) theory of localist educational task groups, Yunker's (1977) theory of professional socialization of police officers, Finch's (1978) theory of the teacher and the change process in schools, and Wolfson's (1974) and Lipnick's (1976) theory of post-Bar Mitzvah religious education, to list just a few.

Asking the question, What is your theory of . . . ? forces the inquirer to attend to issues such as: What is meant by theory? What are the differences between generating and verifying (proving, falsifying) theory? What stance—inner or outer—do you take? What is meant by explanation? What are the boundaries of your case? Of what is your case an instance?

I do not believe that these questions are inapplicable to other research modes, but educational ethnography or participant observation research is an innovative and evolutionary development in most schools of education. The questions take on different meanings in this new context. Trying to justify a different set of activities demands a consciousness of issues that most others can handle by assumption or by appeal to the status quo. Further, in making a judgment of the adequacy of a piece of qualitative research, one important criterion is theoretical coherence of one's point of view. Such internal consistency runs through these metatheoretical issues.

In our first attempts at participant observation (Smith & Geoffrey, 1968; Smith & Keith, 1971), we were confident that what we sought was the beginnings of a theory of classroom teaching in the one instance and a theory of educational organizational innovation in the other. We were operating well within the logical positivist's conception of theory. Some of this had roots in Feigl (1945) and the early debate regarding operational psychology, Bridgeman (1927), and Boring (1945). Zetterberg's *On Theory and Verification in Sociology* (1965) is a simple, coherent account of this position: Concepts are abstractions of reality; some are categorical or class labels, and others are dimensions or variates. Propositions are relations between two or more concepts. Those propositions that are tentative are hypotheses, and those that are more strongly corroborated are laws. When two or more propositions are joined together, one has a theory. Zetterberg described several variations—inventory of antecedents, inventory of determinants, chain structures, and axiomatic formats. We have developed this position in considerable detail in Chapter 1, "The Nature of Classroom Microethnography," in *Complexities*.

Such a conception led us in several directions. We developed glossaries of concepts with theoretical and operational definitions and pictorial models of miniature and middle-range theories. It led us to a conception of explanation, which later we found formalized in Hempel's (1965) covering law model, both deductive nomological (DN) and inductive statistical (IS). The conception legitimated our emphasis on theory generation in the case study, with later verification/falsification in more classical experimental and quasi-experimental designs. Figure 3 depicts this sequence.

Our conception of theory also led us to see educational principles as one piece of social science and social science as part of the larger general fabric of natural laws produced by science. In turn, this was part of a lawful determined universe. This point of view was influenced, illustrated, and legitimated by several books of George Homans: *English Villagers of the 13th Century* (1941), *The Human Group* (1950), *Sentiments and Activities* (1962), *The Nature of Social Science* (1967), and *Social Behavior: Its Elementary Forms* (1961). All in all, this conception provided a very powerful, integrated stance.

In recent years, this intellectual structure has begun to unravel. The unraveling has not been the simple pulling of a single thread; the fabric was snagged in multiple places and in multiple ways. Early on, it was a major puzzlement in *Complexities*.

A serious discontinuity exists within educational psychology. The language of learning theory—Hull, Mowrer, Skinner, or other behaviorists—used to analyze the behavior of children cannot easily be used by the teacher to analyze and alter his own behavior. As we see it, the problem focuses on the pupil as an object, a complex of operant and respondent behavior controlled by the environment, a part of which is the teacher. The child's "rationality" and autonomy are minimized as the program and the reinforcing contingencies are accentuated. The teacher, however, usually is implored to be rational, to plan carefully, to meet the child's needs, and so forth, as though the locus of control lay within himself. The teacher who thinks about his own behavior as a series of operants has difficulty in synthesizing these positions. We believe the issue lies fundamentally in the heart of contemporary social science theory, and we do not propose anything like a basic solution. Rather, we are going to present a way of talking about teaching that has seemed "comfortable" to us at this fundamental level. It has provided a congruence between the experience of observing and participating in teaching and the language available for describing teaching. As we meet the traditional problem areas of educational psychology we will try to rephrase them from this point of view. (Smith & Geoffrey, 1968, pp. 87-88)

This puzzlement is in part the genesis of this chapter and the innumerable activities between then and now.

Snags occurred in reading Bruyn's *Human Perspective* (1963); he makes a case for participant observation as the method of sociology, based on the central criterion that it respects the nature of the subject matter, the human condition. He contrasts it with the "traditional empiricist" position. I found myself doing what I thought was participant observation, yet doing it from a rationale which he saw as a polar opposite. Table 5 is from his account.

TABLE 5
The Human Perspective: Methodological Dimensions

	Inner Perspective (Participant Observer)	Outer Perspective (Traditional Empiricist)
Philosophical background	Idealism	Naturalism
Mode of Interpretation	Concrete procedures	Operational procedures
Conceptualization	Sensitizing concepts	Formal concepts
Description	Synthesis	Analysis
Explanation	Telic	Causal
Principles	Voluntarism	Determinism
Models		
Aims	Sensitively accurate interpretation and explanation of man's social and cultural life	Accurate measurement and prediction of man's behavior

Source: *The Human Perspective in Sociology: The Methodology of Participant Observation*, by S. T. Bruyn (Englewood Cliffs, N.J.: Prentice-Hall, 1966, p. 49).

The Homanian rationale is heavily on the side of traditional empiricism. In addition, Bruyn argued from a broader position in the humanities. The Aesthetic Education Program (Smith & Schumacher, 1972) literature became increasingly salient. The central point, however, remained—Homans and Bruyn, the major exponents of case studies, qualitative observation, and generation of grounded theory, were operating from highly divergent metatheoretical positions.

Quandaries occurred in trying to formalize a concept of explanation. As I read Hempel (1965) and saw the linkages to earlier reading of Feigl (1945), I cheered at the more philosophical underpinnings of Homans and Zetterberg. The major villains that Hempel was flogging were Scriven and Dray, so I went back to their original papers (Scriven, 1959; Dray, 1957). In his characteristic style, a style not known for understatement, Scriven (1959) blithely stated his thesis:

Such, in brief, is the argument that ties together the certainty of explanations with the possession of laws and the possibility of predictions. In its most convincing form, it is due to Professor C. G. Hempel. I refer to it as the deductive model of explanation because it proposes as a criterion for good explanations the deducibility of a statement of the facts to be explained from statements of the antecedent condition and relevant laws. *I have the greatest respect for its powers, its interest, and its adherents, but I shall argue that it is wrong, not only in detail but in conception.* (pp. 444-445; italics added)

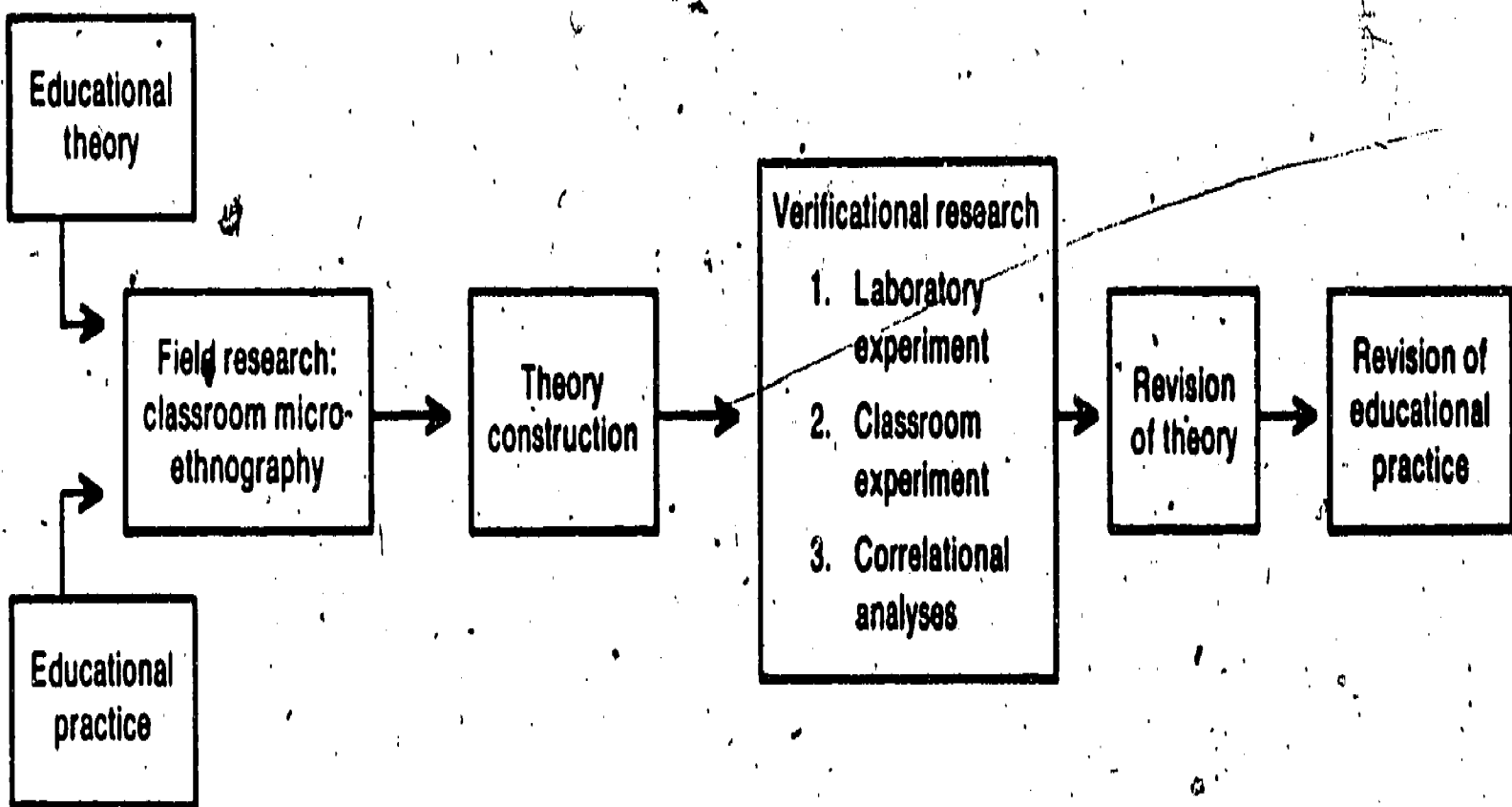


Figure 3. A process model integrating educational research styles, educational practice, and educational theory. (From *The Complexities of an Urban Classroom*, by L. M. Smith and W. Geoffrey. New York: Holt, Rinehart & Winston, 1968, p. 246.)

On the one hand, it is reassuring to find philosophy of science, like other intellectual domains, riven with conflicting positions among major theorists. On the other hand, this is troublesome when one is trying to put one's intellectual house in order. And particularly it is troublesome when one is trying to justify a departure from the norms of inquiry in the educational research community.

In part, too, the unraveling occurred as I read more intensively in the philosophy and social science of science—particularly the splendid series by Toulmin and Goodfield (1961, 1963, 1965). The diversity of what was and is scientific method and theory is startling to behold. Stories that I knew before as isolated fragments from high school and college science were elaborated and arranged into a complex design. Variations in inquiry across subject matter areas and across centuries were pieced together. The incredible celestial forecasting of the Babylonians was dust bowl empiricism at its best. The invention of theory by the Greeks spanned a multitude of "interpretations" of celestial affairs before the current synthesis, or present-day common sense. The discovery of time, the Darwinian revolution, the implication for "the present state of nature and humanity as temporary products of a continuing process developing through time" (Toulmin and Goodfield, 1965, p. 246) challenged the fixity of the 17th-century religious-scientific world view. This broader history intermingled with Snow's *The Two Cultures and the Scientific Revolution* (1959), Kuhn's *The Structure of Scientific Revolutions* (1970), and Ziman's *Public Knowledge* (1968), all accounts of scientific communities and how they function.

At a personal level, I found myself challenged by Professor Beittel, both in conversation and in coming to terms with his provocative work on the production of art. He was doing his own variant of participant observer research; he had read our work as carefully as anyone, and he was chastising us for not seeing the full implications of what we were doing. Our lack of vision concerned the incompatibility of our stance at the root metaphor level of contextualism, and the very important problem of individual creativity. In an important way he provided an artistic educational instance of Bruyn's more general sociological perspective. This he did while we were doing an evaluation of an aesthetic education program and more generally starting a series of inquiries into psychological aspects of aesthetic education (Smith & Schumacher, 1972; Smith, 1975a & b, 1977a). It was timely, troublesome, and stimulating to be told that the world view, the root metaphor, the major polar principle, was incompatible with the logic of the methodology we were using and with the central procedural activities in which we were engaged.

Then there came an internal tearing apart of the fabric by the dominant group of educational researchers. As an educational psychologist, I have been taken aback with a number of my most respected colleagues who have expressed a malaise with the theoretical outcome of traditional educational and psychological research, even when done at a highly sophisticated level (Cronbach, 1975; Jackson; Jenkins, 1974; Sarbin, 1976.) The major statement, however, is Cronbach's (1975) address on receiving the APA Distinguished Scientific Award. He makes several points skeptical of scientific theory in education.

First, Cronbach observes that the aptitude treatment interaction problem seems unsolvable as regards more complex and higher order interactions: "Once we attend to interactions, we enter a hall of mirrors that extends to infinity" (p. 119). Second, he suggests the time element—which decade the data were gathered—as a source of interaction of great importance to educational affairs. Many empirical propositions, especially those involving open systems, have a short half-life. Once again he develops a powerful metaphor to make his point: "It is as if we needed a gross of dry cells to power an engine and could only make one a month.

The energy would leak out of the first cells before we had half the battery completed. So it is with the potency of our generalizations" (p. 122). Third, this view has major consequences for his early work on construct validity, in which he and Meehl imported an epistemological rationale from a logical positivist view of social science as a part of natural science (Cronbach & Meehl, 1956). Fourth, as he shies away from "enduring theoretical structures" and "theoretical palaces," he moves toward "interpretations in context." These are more heavily descriptive and more sharply tied to local situations. He, too, accents the analogy to historical inquiry and to the generation of a perspective or point of view. Finally, as he argues that broad and enduring theories about man in society are unlikely on the horizon, he sets forward two goals for the educational inquirer: "One reasonable aspiration is to assess local events accurately, to improve short-run control. . . . The other . . . is to develop explanatory concepts, that will help people use their heads" (p. 126). More and more Cronbach seems to be moving toward what Bruyn (1966) calls "concrete universals," meanings in the local culture, and "sensitizing concepts," which link the scientist's world to the conventional world.

In an interesting sense these more recent educational psychology points of view seem to come full circle, back to Scriven's early confrontation with Hempel. In his conclusion, as he argues for truisms as grounds for historical explanation, Scriven quoted, then rejected, Boswell: "Great abilities are not requisite for an Historian. . . ." Rather, Scriven (1959) felt: "To get the facts ready to one's hand, to avoid invention in reporting them, to penetrate their meaning and illuminate their presentation it might well be said that these *are* tasks to tax the greatest powers of the human mind" (p. 471). Cronbach, referring to Scriven, made his final point:

The special task of the social scientist in each generation is to pin down the contemporary facts. Beyond that, he shares with the humanistic scholar and the artist in the effort to gain insight into contemporary relationships, and to realign the culture's view of man with present realities. To know man as he is is no mean aspiration.

To someone who has been arguing for "theory generation" as the goal of qualitative field research, this is not a happy state to be in.

Finally, I have been strongly influenced by what I've come to call the Hirst (1966), O'Connor (1973), and Struthers (1971) debate, the degree to which educational theory is mostly scientific, mostly ethical, or some combination of the two. The position one takes on this metatheoretical issue determines quite strongly the logic of the argument one derives from one's data and analysis. If the key terms of the theory—for example, education, curriculum, and teaching—contain value statements in their very structure (as Peters, 1965; Scheffler, 1971; Green, 1971; and Gowin, 1976, among others, argue) then an "objective," "scientific" theory of education is impossible. An educational theory which has ethical components in its core concepts is a very different theory from one which is "scientific." Such a view ripples through such practical items as making recommendations from research studies, dealing with multiple and sometimes conflicting values in evaluation, analyzing divergent interests in the politics of education, and making discriminations among such activities as teaching, instructing, and indoctrinating.

In conclusion, the more involved I became in observational studies, in reading methodological rationales of other observational inquirers, in keeping up with the activities of educational and social science colleagues, and in trying to ground the theoretical rationale in more philosophical conceptions, the more difficulties and irresolvable dilemmas arose. Nonetheless, as I read observational reports, the

criteria I tend to focus on at the theoretical level are these: First, insightful distinctions, that is, novel concepts, propositions, and perspectives that tell me something about the phenomenon that I did not know before. Second, clear definitions of new concepts, at the semantic or theoretical level and at the operational, index, or concrete example level. Third, a cumulating glossary of these ideas within a specific project and across projects, that is within the investigator's research serial. Fourth, the interrelations of ideas into patterns or concatenations, as Kaplan (1964) calls them, or more abstract formal deductive systems, as Hempel (1965) calls them. Fifth, I want the findings to be useful, that is, helpful in solving problems when I'm working in the same broad domain, either as a researcher or a practitioner. All this seems to be a way of saying that the theory should be novel, comprehensive, internally consistent, and functional, a reasonably conservative view of theory.

Metatheoretical Issues: Assumptions in the Phrasing of Results

To ask what has been learned from an observational project is to pose the theoretical-analytical-interpretive issue in a slightly different form. To have a rationale for the manner in which one phrases those results moves one into a series of metatheoretical dilemmas. To be called on to judge observational research proposals, as one might do on an NIE panel, or to judge reports of observational projects, as one might do as a book or journal editor, demands a stand on these issues.

Root Metaphors. Implicit in the structuring of one's results are more general assumptions, stances, and perspectives, analogies and metaphors from which individuals operate and which guide thinking over troublesome spots and implicitly make some events but not others problematic. Stephen Pepper (1942), the philosopher-aesthetician, speaks of these as root metaphors. He argues that four or five of these have currency in contemporary thought: mechanism, organicism, formism, and contextualism. To conceive of human events in terms of clocks and pendula or billiard tables, balls, and cues, is to adopt a mechanistic root metaphor. Behaviorism, in his view, is a psychological theory grounded in a metaphor which solves many problems but has more trouble coping with problems of creativity, of opinion, of choice, of tragedy. Organicism or growth metaphors undergird many counseling and personality theories (e.g., self actualization). Formism or structural metaphors give legitimacy to trait theories and classical individual-difference psychology. Contextualism raises images of historical happenings, events, contexts, and syntheses. Goffman's (1959) brand of symbolic interactionism and Burke's (1945) grammar of motives would be others. In brief, the hypothesis I'm proposing is that a critic, judge, or evaluator brings one or another of these root metaphors to his task, just as the inquirer brings one or another to his task. Most participant observers are probably contextualists. When mismatches occur, the critic or judge defines almost unconsciously the inquiry out of the domain of legitimacy.

Inner vs. Outer: Observer Stance and Theoretical Perspectives. When Geoffrey and I first began thinking about what we were doing methodologically, we began to talk about our "inside-outside" stances. This seemed to make uncommonly good sense in understanding what was going on in the classroom. I was the objective outsider; he was the insider, privy to the entire system. Later we were to find that Gold (1958) and Junker (1960) had a taxonomy of such roles, that Bruyn (1967) spoke of involvement and detachment, and that Powdermaker had a book entitled *Stranger and Friend* (1966). She stated the task for the outsider in lucid prose in the preface:

To understand a strange society, the anthropologist has traditionally immersed himself in it, learning, as far as possible, to think, see, feel, and sometimes act as a member of its culture and at the same time as a trained anthropologist from another culture. This is the heart of participant observation method—involvement and detachment. Its practice is both an art and a science. Involvement is necessary to understand the psychological realities of a culture, that is its meanings for the indigenous members. Detachment is necessary to construct the abstract reality: a network of social relations including the rules and how they function—not necessarily real to the people studied. (p. 9)

Once again, we find that the kind of "learnings" one desires from one's inquiry keep tugging at the kind of data. For Powdermaker, "meanings for indigenous members" and "construct the abstract reality" constitute the twin goals. Others (Bruyn, for instance) stay more with the meanings for the participants, while some (Homans, for instance) are more behavioristic natural scientists looking for general truths and hypotheses. I have never lived comfortably with the dichotomy between the inner perspective—the world as viewed from the point of the agent—and the outer perspective—the agent viewed as one part of the natural world.

A resolution of synthesis, if it be that, seems awkwardly simple, perhaps overly simple. It lies in the continual transformation of the internal perspective through knowledge from the external perspective. For instance, in our early study of an urban classroom we approached the setting, in part, as descriptive behaviorists. The analogue between the Skinner box and the classroom box was very real. Equally, Geoffrey's point of view, especially his decision making, seemed very critical. Our hope was, and is, that insofar as we could say some important things about the regularities of his classroom, regularities that were generalizable to other classrooms, other teachers could be taught to think and act in terms of these regularities. Their internal perspectives would be enhanced by such knowledge.

This same position is reflected in a point of view about consulting which derives in part from Gouldner's (1961) comments on the theoretical requirements of applied social science. First, one needs to carefully understand the client's purposes, internal perspective, and statement of the problem. Second, these ideas must be translated into one's own theoretical point of view and solved in one's own terms. Third, the "results" must be translated back into a framework useful to the clients. Usually a fourth step occurs, one of mutual adaptation. The experience teaches both parties things they hadn't known before—enlarges and differentiates their repertory of ideas.

If I read the Toulmin and Goodfield (1961) point of view correctly, they are arguing that the conventional wisdom, the common sense of an era, has evolved over the recorded history of man. As they say in regard to the fabric of the heavens, "Common sense is a powerful mould. . . . One century's common sense is an earlier century's revolutionary discovery which has since been absorbed into the natural habits of thought" (1961, pp. 15–17). People with commonsense ideas that heavenly bodies are of different orders, such as stars, planets, and meteors; that atoms are not the ultimate particles sought by the Greeks; and that human history antedates Adam and Eve are in some important respects different from people of earlier generations. The most important aspect of this difference is that outer perspectives developed by cumulating applications of intelligence and rationality to problems have created social structures which in turn create individuals with different internal perspectives. Scriven (1972) takes an even stronger position and argues for sidestepping the entire debate: "I want to suggest that once again we should be willing to forget the dichotomy of

external/internal, subjective/intersubjective, and think of these as claims that both require and refer to internal states and external ones" (p. 114). All of these arguments seem to concur with, but go a step further than, Campbell's discussion of qualitative knowing, which we cited earlier.

Miniature Theories vs. General Theories. Because case studies of classes or schools often have a holistic or systemic quality, they tend to get caught in dilemmas regarding the scope of the theory. Various labels have been coined, such as miniature theories, middle-range theories, and substantive theories, to contrast with abstract theory, formal theory, or general theory (Glaser & Strauss, 1967; Merton, 1957; Zetterberg, 1965). In our work we have tended to solve the problem in several ways. First, we accent the building of pictorial models of smaller pieces of the educational world under study. These might be described as miniature theories of pupil roles, such as court jester, or pupils on contract (Smith & Geoffrey, 1968), or miniature theories of facade and individualized curriculum and instruction (Smith & Keith, 1971). The major concepts within these miniature theories become parts of larger and more abstract clusters within the same research study. Increasingly, and with some difficulty, we are attempting to link them across studies. For instance, in the *Alte* investigation in 1977 I picked up the concept "idiosyncratic styles of teaching," which had come up almost tangentially a decade before in our apprenticeship study (Connor & Smith, 1967; Smith, 1972). The hope is that these larger integrations will culminate in a more general theory of education.

Pattern Explanation vs. Deductive Explanation. If one can believe the philosophers of science (Hempel, 1965; Kaplan, 1964; Scriven, 1958, 1959), a large controversy exists in the phrasing of abstract social science results. We got mixed up in it in several ways. First, the Homanian approach, which is essentially grounded in a deductive nomological rationale, was one of our guides. Second, Becker's (1958) early paper on problems of inference and proof in participant observation suggested a four-step procedure: (1) selection and definition of problems, concepts, and indices, (2) checking the frequency and distribution of phenomena, (3) construction of social system models, and (4) presentation of results. It, too, was influential and pushed us toward patterns, configurations, and concatenations. Early on, Geoffrey, Keith, and I struggled with the various diagrams, models, and miniature theories in *Complexities* and *Anatomy*. We tried, unwittingly at times, for more abstract, deductive, covering law statements and at other times for more concrete, systemic, configurations or patterns. The dilemma has remained. We have found each mode to be informative at different points as we reached for understanding.

Educational Rather than Social Science Theory. Traditionally, and particularly in its research efforts, education has borrowed from the social sciences. The methods, measures, apparatus, and ideas of anthropology, sociology, and particularly psychology have been brought to bear on the teacher, the classroom, the school, and the curriculum. At this time it seems appropriate to ask whether education has profited substantially from the stepsister relationship, and whether it may now be time to take seriously such terms as teaching, curriculum, steering group, lesson, and recitation and to build a genuinely educational theory with core primitive terms, derived terms, and sensitizing concepts of its own. Participant observer research seems uniquely suited to this task because of its efforts to understand events in a culture and system from the point of view of the practitioners in the system. Ferreting these out, codifying the shades of meaning, building them into configurations and propositions, using them to solve significant problems seems a worthy objective. A vigorous application of such a criterion by journal editors or funding committees would change the nature of educational jargon and maybe eventually the phenomena themselves.

Social and Educational Theory: Natural or Artificial. Basic to the formulation of one's results is a belief concerning the formulation of social science generalizations as natural science laws or as more artificial or artifactual principles dependent on human institutions which are constructed for individual and collective purposes. In a sense this seems a form of emergence, that is, living organisms and events are different in some important ways from inorganic events, and human beings are different in some important ways from other animals. As one moves into social events of human beings, the openness and indeterminacy loom larger and larger. Natural law seems less and less applicable, so the argument goes. Within educational theory Cronbach (1975) and Gowin (1976) have voiced a variant of this more explicitly than most of their fellows.

The is/ought problem is solved classically by splitting science and ethics, with science trying to describe the world as it "really is" and with ethics trying to clarify the good or the ideal, the world as it "should be." As social theory is conceived more toward the artificial or artifactual, it often takes on aspects of practical theory or a theory of action (Schwab, 1969, 1971, 1973). This raises old arguments that are sometimes posed as historical explanation vs. scientific explanation. Dray (1957) and Scriven (1958, 1959, 1972) have had a running argument with Hempel (1965) for several decades over these issues. In a recent paper, within a long series of essays attacking the more formalist position of the logical positivists, Scriven (1972) argues again the nature of historical knowledge as the prototype for education in particular and social science in general. He is referring to "weak knowledge claims . . . a knowledge claim of a rather different kind from the usual ones." He argues:

One would thus expect it to be the norm in history and, if this approach is logically sound, it does something to rescue history from the choice between (usual conceptions of) the Scylla of science and the Gharybdis of literature. We can, for instance, answer the question, what do we learn from history, without having to produce absurd or trivial laws, or bare particulars about the past, or murmur mysteriously about deepening our understanding of man. What we most importantly learn from history is a range of possibilities—not of probabilities, not of certainties. And of course these are not mere possibilities, of the kind that one has in mind when one says, Oh, anything is possible! They are significant possibilities, ones that have shaken empires or cabinets before and may do so again for all that we know to the contrary. They are thus most deserving of our respect, and with our knowledge of them we can plan more rationally for the future. If we wish to make a certain outcome more likely, then we can try to bring about those conditions which on previous occasions, not demonstrably irrelevant to the present case, did bring about that outcome. If we wish to prevent an outcome, what we can do is to make it less likely; we try to eliminate those circumstances which have in the relevant past brought about this result or repeat those circumstances that have previously frustrated it. We rarely have much idea about how much effect such actions will have, speaking precisely, but we sometimes know that they have a "good chance," or are a "desperate hope," and to suppose that we shall ever be much better off than that in human engineering a history-conscious world is pipe dreaming. The sad thing is that we could have done so much and done it so much better if we had been willing to learn the lessons from history that are there to be learned; instead of going in search of some Holy Grail whose contents would give us the same kind of predictive reliability in history that we have in astronomy. (p. 115)

If I understand him correctly he is arguing the more general epistemological case of knowledge in a theory of action.

In summary, educational research workers differ on a number of often implicit dimensions and configurations, which might be called metatheoretical issues. A half dozen of these seem particularly important:

1. The root metaphor within which one works—mechanical, organic, formal, or contextual.
2. The inner or outer perspective one chooses, that is, a stance from the subject's point of view or the outside observer's point of view.
3. A theory which is more limited in scope and time to a local context versus one that is more general.
4. A level of abstraction that is more descriptive and concrete or more abstract and interpretive.
5. A model of explanation that is more covering law versus one that is configurational or contextual.
6. A theory that is more action oriented and more ethical versus one that is more descriptive and analytical.

It is my contention that a reader of participant observer reports, as he positions himself at one or another of the poles in these-metatheoretical dilemmas and as he treats these positions as value laden—good, appropriate, desirable—will make varying judgments on the quality of any particular piece of research. It is my contention also that while these issues separate various ethnographic researchers from each other, they also represent major differences between the ethnographer and the larger community of educational researchers. As such they are problems needing more general attention in the educational research community.

CONCLUSIONS

This essay has been directed largely to the educational research community, perhaps most thoroughly represented by AERA. Most basically, the hope has been to widen and deepen the discussion of a broad methodological stream of inquiry—educational ethnography, participant observation, and other case studies. More by implication than direct analysis, the assumption is that the dominant paradigm—experimental, quantitative, positivistic, and behavioral—has been too restrictive to cope with the ideas, the problems, and the interests of what is called education and of people who call themselves educators.

More positively, the analysis and its several lines of argument have tried to make several major points. A large, interesting, and provocative literature, both substantively and methodologically (Tables 1-4 and the reference list) exists within this field study tradition. As the activities reflected in those tables engendered conversations and communications, individuals began to coalesce in conferences, groups, communities, and invisible colleges. Witness, for example, the Cambridge Evaluation Conferences of 1972 and 1975 and the recent publication of *Beyond the Numbers Game* (Hamilton et al., 1977), a reader in alternative methods of educational evaluation. The elements in a theory of research methodology, as items or patterns, can be viewed as group norms or mores of those evolving communities. Over time, the larger research communities and subcommunities evolve and change, as in the development of Division G, Social Context, in AERA and the Council on Anthropology and Education in AAA. In time, the theory of research methodology, as with any theory, evolves and changes, perhaps on occasion dramatically enough to be labeled a revolution, as Kuhn calls the paradigm shifts.

Second, a reflexive overview of the cognitive processes in field work suggests a perspective on the methodology. It is one person's idiosyncratic "how to do it." It, like the methodology, builds on one participant's actions and observation of the process, reading about other persons' observations, and then describing, analyzing, and interpreting that experience. It is its own kind of grounded theory of methodology. Experientially, the phases and the discriminable items within the

phases, are very real. They have a public quality in being sharable, communicable, and meaningful to other field workers. They seem to help our students in learning to do similar work. They seem to lead to knowledges and understandings that are useful to a variety of persons engaged in educational activities.

Third, and more specifically within the educational ethnographic, participant observation tradition, the essay presents a patterned analysis of the genre of research. The major domains considered were at four levels of abstraction, data, descriptive narrative, theoretical, and metatheoretical. The framework presents a way of talking about any piece of research in the tradition. Such a perspective is one step toward a guide to evaluation. Audiences can discern relative kinds and amounts of attention to data, to narrative, to theory in a piece of work. If they value portrayals more than conceptual models, for instance, then one piece will be judged better than another; conversely, if they value model development and they find only a portrayal, the evaluation will be different. Each of the levels of analysis contains several discriminable subissues. All of these are linked in multiple and sometimes contradictory ways to other research traditions in education, social science, and philosophy. Overall the framework suggests the kinds of things the practicing inquirer might consider as he is developing his own line of research. Particular positions of the author were presented in considerable detail as one configuration of possibilities. For now that seems enough.

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3.2 Accidents, Anomalies, Serendipity...

Accidents, Anomalies, Serendipity
and Making the Common Place Problematic:
The Origin and Evolution of the Field Study "Problem"

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1. INTRODUCTION

The idea I wish to explore in this essay is the genesis of field study research problems. The data and format are a case history of our field study efforts over the last fifteen years. The basic interpretation falls somewhere around the title of the piece - accidents, serendipity and making the common place problematic.

A number of reasons suggest that this might be a worthwhile exercise. First, the issue seems to be relevant to one or another of the conference topics - the validity and usefulness of field study knowledge, the ethics and political aspects of field work, and the training of field workers. In my experience, many students learning field based methodologies have great difficulty "stating their problem." At the same time many critics of field based methods, who come from a more hypothesis testing perspective and tradition, cannot cope with field method modes of problem statements. This, in turn, prevents the integration of the several traditions into a larger perspective on educational research methods. Further, the nature of the statement of one's problem has a great deal to do with the kind of interpretation, generalization or hypothesis development one makes; that is the phrasing of the results of the field study. This seems a short step from the kind of use one might make of the results in thinking about other issues. In an important sense, the origin and initial statement of the problem are critically important for the discussion, negotiation, and contract one makes with the individuals in the setting under study - the heart of ethical issues in field work. In addition, along the way, I hope to make a case for the utility of field research, the politics of change, based upon the origins of the problems..

Finally, the idea for this essay grew concurrently with a related essay, "An evolving logic of participant observation, educational ethnography and other case studies" (Smith, 1979). In the latter, a series of arguments were presented. (1) Research methods are norms of particular groups of scientists (Kuhn 1970, Ziman 1968); (2) The norms of the educational research community (e.g., AERA) have been reflected in and subsequently determined by the Campbell/Stanley (1963) paradigm; (3) Other research groups--especially in sociology and anthropology--have long and strong traditions of qualitative naturalistic observational research. (4) The intellectual processes of this qualitative tradition can be described and analyzed in several phases: early (origins of problems, guiding models, foreshadowed problems), middle (concrete images, interpretive asides and conscious searching) and final analysis and write up (case as an instance of a general category, comparing and contrasting, antecedents and consequences, and collapsing outlines); (5) Criteria for judging qualitative observational research can be organized at four levels: 1. the data level, 2. the descriptive narrative level, 3. the theoretical analytical level, and 4. the metatheoretical level. One of the most troublesome of those issues lies in the genesis of the research problem in field studies. This paper attempts to clarify that further.

A word seems necessary on the case study strategy of looking back on one's own efforts. I have strong commitments to cumulating cases (Smith 1971, 1977), to reflexivity in research, that is, the methods and findings should play back and forth into all aspects of one's professional life (Smith 1979), and to the explication of research serials (Smith 1974, 1977, 1979). The latter feeling has been stirred by Beittel's work (1974), and was accented this summer when I spent six weeks at CARE, the Center for

Applied Research in Education, at the University of East Anglia. The opportunity to read accounts of the Humanities Curriculum Project, SARARI, UNCAL, and the Case Records Archives Project, and then to talk almost daily and often at several hours at a time with the various individuals who had been involved in some ten years of field research on curriculum and teaching--development, evaluation, inservice, dissemination--was very instructive.

(e.g., MacDonald & Walker 1976). While that is another story, it strengthened my belief in the importance of reporting on a particular research serial.

Figure 1 contains our various projects arrayed on a time line, clustered as to setting, system, and substance under study and with source of support. Hopefully this will provide an overview, presage some of the more general statements that I want to make, and form the background for a contextual analysis.

Insert Figure 1 about here

Substance, Settings,
and Systems

Districts &
Multiple Or-
ganizations

Schools as
Organizations

Anatomy

Curri-
cula

Patterning
(1966)

Teacher
Educa-
tion
'68-'72

Rural
Highlands
'68-'69

New
Math
'69-
'70

Rein-
forcement
Project
'70-'71

AEP Pilot
Trials
'71-'72

8 Day
Week
'73

Classrooms
& Teacher

Complex-
ities (1963)

Teacher
Awareness
'66-'67

Teacher
Plans
'67-'68

Mrs. K's
Drawing
Class '75

Teaching
Effectiveness
1976

128

Indivi-
duals

The Child's
Aesthetic
Experience
'75-'76

3 Arts
Project
'75

Two Week
Show Tour
'76

Method-
ology

Micro-
ethno-
graphy
'67

Co, Bug, Co
'68-'70

Grounded
Theory
'69-'76

Cumulating
Cases '71

Model Building
'76

Evolving
Logic
'78

Essays on
P.O.

Years

1963

1964

1965

1966

74-75

1976

1977

1978

1979

Support

OK

CERIAL, Inc.

Pullbright

MSP

MEM

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Figure 1 A Chronicle of Research Problems

139

140

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2. THE CASE STUDY: DESCRIPTION AND INITIAL INTERPRETATIONS

2.1 The Initial Studies

While the origins of The Complexities of an Urban Classroom had its own unique set of particulars, its latent characteristics were a prototype of the origins of most of the later studies. In a Washington University summer course entitled "The Classroom as a Social System," we were discussing a number of pieces of research, their key concepts, and their measuring instruments. As a class exercise everyone took the Minnesota Teachers Attitude Inventory (MTAI). One of the class members, William Geoffrey, upon handing it in commented, "This is the way I feel but it's got nothing to do with the way I teach." I probably said something like "Oh," as I recovered from the neatly inserted needle and we began a conversation. Along the way he suggested that I ought to come down into the inner city and see what it was like in the real world, another bit of commentary on the kind of middle class educational psychology we had been dealing with. That fall I did spend a few days there, and began the round of entry conversations that led to the following fall's intensive observation.

The beginning of that problem possessed several key aspects. First, an experienced teacher believed some untapped (at least by me) important events were going on around him. While we didn't know it then, this stance of problem identification has been called more recently, practice oriented inquiry (Atkin 1973). He distinguishes it from discipline oriented inquiry. Insofar as any ideas and insights can be garnered they should have relevance to the domain from which they came. That seemed tacitly important then. Now those ideas are linked with several of Glaser and Strauss' (1967) general points: the generation of grounded theory vs. the verification of

theory and substantive theory in contrast to formal theory. If knowledge utilization is a long-term goal these seem important antecedents.

In our initial attempt to formalize "the problem" we tied it to some developing concerns in educational theory in our initial proposal:

To this investigator's knowledge there has been no significant break-through in conceptualizing a middle class teacher's coping with a class of children from a lower social class. Recently, strategies for dealing with lower class children have been suggested in a number of sources. They are impressive in their vividness, their variety, and, to the present, their lack of synthesis. Becker (1951, 1952) suggests that the teacher must be showy, dramatic, and acceptant of minimal effort. Watson (1962) suggests "The starting point is respect. Nothing else that we have to give will help very much if it is offered with a resentful, contemptuous, or patronizing attitude (p. xi)."

Riessman (1962) comments:

"She can concentrate on those values which she holds in common with the pupils and ignore the differences . . . She can ignore value questions entirely and focus on teaching basic skills and knowledge . . . There is one more option, and this viewpoint underlies the present book. The teacher may, in a designated area, explicitly and openly disagree with a value bias like anti-intellectualism. If she does this directly and honestly, not snobbishly or manipulatively, she may be able to sway students, and may also be more at ease in her relationship to them (p. 82)."

And then he states, almost as a second thought, what seems to this investigator a most important corollary:

"Of course, disagreement, to be fruitful, has to take place within a framework of general acceptance, and so the teacher, in order to be effective in opposing certain beliefs, has to be able to share, or at least respect other sentiments (p. 82)."

It is precisely at this point, the understanding of the context surrounding the selection and use of a strategy, that the present research will focus (1963).

We also made some comments about what seemed to be relevant social psychological literature.

In recent years the concept of group norms has become central in the theories of many social psychologists and sociologists (Sherif et al., 1956, 1961; Schacter, 1951; Homans, 1950; Thibaut & Kelley, 1959). In his extensive analysis of the structure of group norms Jackson (1960, 1962) has presented a return potential model which incorporates a number of phenomena related to norms. One aspect of this is norm correspondence or non-correspondence. Essentially this is the degree of similarity in the values or schemata of an individual group member and the more general norm of the group. Jackson's analysis has had obvious productive consequences in general social psychology. It seems to offer a point of departure for a number of important educational and classroom issues relating the teacher's behavior with the classroom group.

It is our hunch that this problem may yield conceptually because it seems to be an instance of the more general case of non-corresponding normative structure. If we can extend Jackson's theorizing, if we can collate the findings of such relevant literature as Merel's (1949) work

with child leaders and "institutionalization," Lippitt's (1958) modification of Lewin in the Dynamics of planned change and Coch and French's (1953) field experimentation in "Overcoming resistance to change" then case study observations of a teacher working with such a problem should be a fruitful addition to educational literature (1963).

In essence, we mixed together two kinds of literature. That's created some confusion which only recently we have begun to straighten out. But that's running ahead of the story. The important point that's assumed in the brief quotes is the assumption that theory--educational or social psychological--should help solve problems of the order "a middle class teacher's coping with a class of children from a lower social class." That, too, has been more implicit than explicit, but it has been a major element in framing problems and in organizing results of the field inquiry. Gouldner's (1961) "Theoretical requirements of the applied social sciences" has been helpful in making that part of the perspective conscious.

That proposal was not funded, and as we heard later, on the grounds that one can't generalize from an N of 1. But Sandy Charters, who was Acting Director of the Graduate Institute of Education that Spring urged me to go ahead and he cut a course from my teaching load to give me some extra time. Later, as intriguing data were coming in, we rewrote the proposal for a small contract from OE, which was funded, mostly for typing help with the field notes and some time released from teaching summer school. At this point the problem evolved another step into "Toward a model of teacher decision making in an urban classroom." The general objectives were stated in the following form.

The fundamental objective of this research is the construction of models and theory to analyze an important practical educational

problem in contemporary society. The purpose is not to verify hypotheses. The practical problem surrounds a middle class teacher's attempts to work with a group of children from an urban slum neighborhood. The theoretical framework which is being formulated is a critical synthesis of such social system concepts as external system, internal system, practical equilibrium, norm, interaction, activity and sentiment, and of such decision making concepts as choice, fact and value propositions, alternatives, consequences and effectiveness.

(Dec. 1963)

The tentative generalizations which were evolving: (1) resources are needed to do the work one wants to do and (2) one must phrase the problem in ways that meet the specific objections and in ways which are compatible with the widest range of one's peers.

The abstract of the procedures contained several items.

Standard participant observation procedures¹ for an exploratory field study of an ongoing institution have been utilized. The principal investigator has spent a full semester all day-every day (His University commitments have been lightened; his classes meet late afternoons and evenings) in direct observation of a single elementary school classroom in an urban slum. The research associate is a regularly employed public school teacher. The field notes of direct observations and informal interviews with pupils and teachers form the basic concrete data which are to be analyzed. To the investigator's knowledge such intensive observation by both an outside observer and by the teacher of the process of development of a single classroom from its beginning

¹ Later Paul Pohland and I (1974, 1976) came to the conclusion that no such entity as "Standard participant observation" exists.

to its semester's conclusion is unique in the educational literature.

(Dec. 1963)

Tacitly, then, in the last sentence, we were on to an important issue, the intertwining and complementarity of problem and method. Now I would argue that all day/everyday, inside/outside, single classroom, beginning to end of semester really elaborate "the problem," that is they accent latent theoretical issues. We didn't see that then; now it seems a very important point in the origin and evolution of field problems. Hence the need for the continuing rationalization of problems and methods within the same conceptual framework.

Finally, the problem evolved away from many of our more specific theoretical ideas and focused on the more general theme of a middle class teacher coping with a group of lower class children. Ultimately as the subtitle of Complexities indicates the final resolution was "Toward a General Theory of Teaching." Now we are more prone to say something of the order of "a description and analysis of . . ." whatever setting or set of events is central. That phrasing permits the widest latitude of investigation and legitimizes the evolution of the problem which seems always to occur. This can be hazardous if the researchers do not have some more specific initial themes, hunches, or foreshadowed problems to help guide themselves.

While at the Washington School, two anomalous events arose which typify the general point of making the common place problematic or dramatic. First, I was struck by the importance of a faculty peer group in Geoffrey's life in the school. It was truly a reference group with a set of norms which were influential in his perspectives and his decision making. I wondered how it had arisen. While these reflections were salient, a chance event

occurred. Larry Iannacone who was Director of our Center for Field Studies had been approached by two Administrators from the Milford School District who were beginning a new school, innovative in design, administration, staffing, and curriculum. Larry was too busy to do the work himself and invited me to lunch with Shelby and Cohen to talk about it. When I heard that one element of the design was "a faculty new to each other," that is, they were coming from all over the midwest I knew I had a setting for the genesis of a faculty peer group. In my own mind, the decision was made, only the details had to be worked out. In a proposal to OE, the problem and objectives were phrased as follows.

The problem to be studied in this investigation contains several components. In its most general aspect, we are trying to capitalize on a rather unusual naturalistic event, the building of a new and uniquely designed elementary school building.

While in a general sense, the question, What happens in such a novel situation?, is the focus of the research, the more specific problems to be analyzed are: (1) the development of the faculty social system; (2) the principal's role vis-a-vis the faculty social system; (3) the teachers' innovations in instruction; (4) the development of the school wide pupil social system.

1) In a prior investigation (Smith, A model of teacher decision making in an urban classroom, 1964), we were impressed with the importance of a faculty peer group as an element in the classroom teacher's decision making. The influence it exerted was not widely recognized in the building. This clique had been in operation for a number of years. In this study we will have an opportunity to observe the formation of an entire faculty system (18 of the 23 teachers are

new to the school district), to see in what ways it is a totality, what and how cliques are formed, the influence processes, and the resulting impact, consonant or dissonant, on the teacher.

2) In the proposed study, the principal's decision making role will be a focal point in light of the novel building design, the demand for instructional innovation, and the majority of teachers new to the system. By capitalizing on these events, which should highlight the issues, we should be able to criticize and extend the theory of decision-making as it has been applied in education, e.g., Halpin, Administrative Theory in Education.

3) Typically, teachers entertain few dramatic changes of style, curriculum content, or classroom organization. The present setting in many ways is optimal for encouraging creativity in these areas. Such innovations with pupils lie at the heart of alterations in pupil outcomes. Observation should lead to rethinking of models of teacher-pupils relationships. These models will be broader than many current ones for they must encompass some of the "team" aspects and also the novel physical resources.

4) The pupils' reactions in their new environment will be observed carefully. While one major focus will be on the developing sentiment toward the new school, such activities, as independent study and social interactions, will be considered also. Geographically, the large library-study room and the covered playground area will be the arenas for initial observation.

In short, while the study focuses on the impact of a unique school plant, and several specific areas of investigation have been suggested, the principal investigator's bias leans toward the broader

goal of the development and integration of social science theory in education. In this regard, an attempt will be made to synthesize role theory, decision making theory, and social system theory as they explicate the functioning of this school. (1964)

The original "problem" became one piece of a larger "What happens in such a novel situation?" The uniquely designed building seemed the strongest way to present the problem; we titled the proposal "Social psychological aspects of school building design." Such conversions of problems seems to occur throughout much of our work as we make best estimates of what is likely to make the work supportable. The limits of this, that is the ethics of such tactics need to be explored theoretically and empirically.

Once again the principle: "With a little help from one's friends," this time a faculty colleague rather than a student. Second the problem was on my mind, I was on the lookout for a setting to see an instance or case unfold. Third, when the setting came along we were able to recognize it, to grab hold of it, and once again to obtain small contract funding. The initial problem of the faculty peer group in effect was "an idea looking for a setting." This I think is an unusual way of stating the problem. It's not a hypothesis to be tested in the usual sense. Yet it is a testable idea in another sense. The trick seems to be finding a setting where the idea will have a fair chance of playing itself out and where we will have a fair chance of seeing it played out. That seems close to what Cartwright and Zander (1968, pp. 32-33) refer to as a natural experiment. Further and perhaps even more important is that the problem changed once we were in the setting. This was reflected in the proposal which was read and approved by Milford and Kensington administrators. The setting took on a life of its own; anomaly after anomaly arose as the events in the life of

the school came and went. Eventually the study became an Anatomy of Educational Innovation: An Organizational Analysis of an Elementary School.

Not only were mundane common place events problematic, but they were on the way to becoming dramatic.

Also, at the Washington School, I met an apprentice teacher from City Teachers College. In the course of getting acquainted, he commented about his teacher training apprenticeship. He spent two weeks in kindergarten, two weeks in first grade, two in second, and so forth on through the 8th grade. Each two weeks had a regularity which ended in having the kids alone all day on the second Thursday. To everyone else brought up in the system it was "the way it's sposed to be;" to me it was an anomaly, incredibly different from Washington University's program and any that I had seen before. Two radically different social structures with the same professional goals had to be interesting. Bill Connor, who then ran the GIE program, and I had been talking in seminar, about collaborative efforts. We decided that it might make a vehicle for those efforts. From that came Patterns of Student Teaching. Later I was asked by Bruce Joyce to do a paper on "Classroom Social Systems and Teacher Education" (Smith 1972). In that, I blended ideas from Complexities and Patterns into a synthesis, reflecting some things I had been wanting to say for some time.

This case suggests several additional aspects of the origins of field work problems. Not only is there a flow of ideas looking for settings, colleagues whose interests overlap at various points, and resources to help do the work, but also there's a continual search for publication outlets. The OE final reports always were "finished" well enough to circulate to interested friends and colleagues. Usually they became take off points for AERA presentations and more formal publications as articles, chapters,

monographs or books. On each of those occasions, the "problem" evolved once more as particular themes for particular audiences at particular times arose were redrafted and were presented. This quality of fluidity as one construes and reconstrues problems, data, and interpretations has been more tacit than conscious. Recently, we have begun to accent personal constructs and contextualism, as Kelly (1955) and Sarbin (1977) have elaborated them, as basic to a more general theory of education.

2.2 The GEMREL Relationship

Initial Studies: In the mid 1960's OE's Labs and Centers Program was getting underway. I was fortunate in getting involved with the beginnings of GEMREL, Inc. and fortunate that the Director of the Lab was Wade Robinson, who was interested in and supportive of our work. Whether this was a happy accident, or serendipity, that gift for finding valuable and agreeable things not sought, I'm not sure. But it was the beginning of a ten year relationship that was most helpful and productive for me. It solved several major problems: first, it provided resources--part of my salary, usually half time to cut my teaching load, a half time assistant, usually a Washington University Ph.D. student, and a part-time secretary. Second there was an unusual degree of autonomy in problem selection and in choice of method. Third, the Lab was on to practical problems or applied problems in curriculum development and evaluation, classroom teaching and school innovation. All in all a remarkable blend of supportive conditions.

With Paul Kleine I began a specific project on the antecedent and consequences of Teacher Awareness, which grew out of a concept in Complexities and which was related to some interests he had in cognitive differentiation and cognitive complexity. Later, that problem was redefined into two larger

problems: substantively it became part of a larger program I came to call "The Psychology of Teaching." Methodologically it became part of a larger attempt for a rapprochement in qualitative and quantitative methods.

Substantively, Complexities contains dozens of concepts, literally hundreds of hypotheses, and a number of miniature theories of aspects of teaching. One of these concepts was "teacher awareness," the degree of knowledge the teacher possessed of the children in his/her classroom. Its antecedents and consequence seemed an important substantive problem in its own right. Later it seemed an appropriate step toward building a verified theory of teaching. Methodologically, the problem was one instance of testing a large strategic model in how to approach inquiry in teaching. Figure 2, from Complexities shows its location.

Insert Figure 2 about here

The point I'm trying to make here is that problems are encased in larger problems and that a reconstrual process is going on constantly. This can't be interpreted at the level of the discrete project; its significance is apparent only in the context of the serial. The serial, however, is evolving at the same time. As with the cycle in Zen and the Art of Motorcycle Maintenance, it's a vehicle on which is always working on. That complicates inquiry into the origins of problems. In short, Klein and I were testing several hypotheses with varied measuring instruments with 69 teachers and hundreds of children. At the same time, we were testing, by way of a single instance, a part of a general model of the relationship between qualitative and quantitative strategies in educational inquiry.

In the meantime, I was looking for a setting to explore the "intellectual

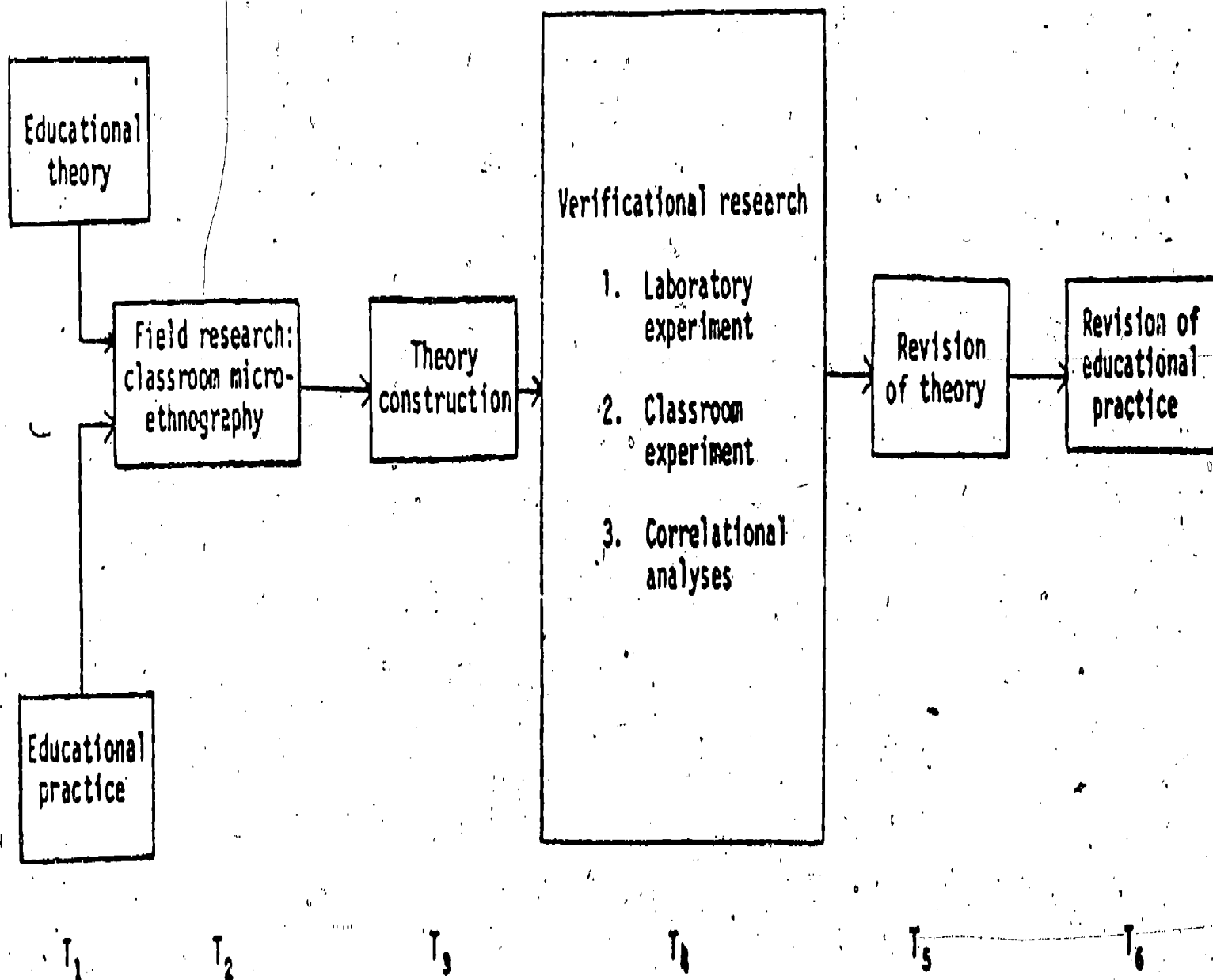


Figure 2 A process model integrating educational research styles, educational practice, and educational theory. (From Smith & Geoffrey, 1968, p. 249).

life of the classroom" as a companion piece to the more "social life" emphasis in Complexities. Methodologically the possibility of exploring the quantification of classroom interaction, another part of the qualitative/quantitative rapprochement problem was continuing. I ran into a former student, Pat Brock, who was just finishing a set of junior high textbooks in general science, Patterns and processes of science (1969). It looked like an opportunity to collaborate productively. From that came a major methodological piece, "Go, Bug, Go": methodological issues in classroom observational research and now after a decade we've almost finished, Teaching general science: a story with some commentary (Brock and Smith, In process). But for us here, the problem statement is most critical. Our initial phrasing of the investigation in the context of CEMREL's Psychology of Teaching project was stated broadly--and vaguely--as follows:

Expected Outcome: To study how teachers plan classroom activities, how they behave in the classroom, and how they work with groups of children. These studies should contribute a more general understanding which will have theoretical and practical utility for the teacher in the classroom and for teacher training.

While the setting and funding of the research had shifted, the specific substantive problems were serendipic offshoots of earlier problems. Often they were a function of interactions with individual colleagues and students. At another level they were part of a larger evolving concern with a "general theory of teaching." Even one phrasing of that as a "psychology of teaching" was an attempt to respond to organizational concerns for a more general statement of our activities. In addition the strain over the legitimacy of participant observational methods became a problem in itself, the rapprochement between qualitative and quantitative method in classroom

interaction. Those larger problems might be labeled better as themes recur continuously through the serial. Most recently in the Monterrey Conference (Smith 1977) and an RRE chapter (Smith 1978) I returned to both. Finally, the perspective, the analysis, and the data raised by Ken Beittel in his Alternatives for Research in Art Education presented an image or model which has guided much of my recent thinking. That suggests an additional and important point in the formulation of a problem--a parallel or analogue that one can return to for guidance in what one is trying to do and for confidence that it might really be legitimate.

Curriculum evaluation: Organizational forces at CEMREL, in the sense of tasks needing doing and tidiness of organizational charts led to a series of studies best classified as curriculum evaluation. It started with the Rural Highlands study, an evaluation of a computer assisted instruction program in Appalachia and continued through a series of observational studies of CEMREL's major curriculum programs, Comprehensive School Mathematics Program (CSMP), the Instructional System Program (ISP) and the extended pilot trial of the Aesthetic Education Program (AEP). From my point of view CEMREL was supporting my research program; from their point of view I was working on the central evaluation staff as a semi outside-independent evaluator. During these years, the exciting "problems" were first the genesis of CEMREL's "3 legged evaluation model," with Howard Russell and second the chance to explore the limits of participant observation in the context of curriculum development, evaluation, and dissemination. Concurrently at Washington University, Harold Berlak and I were sharing a seminar and several students who were doing formative evaluations of his new social studies curriculum.

Paul Pohland and I began the Rural Highlands project as an extension of issues in the psychology of teaching, curriculum evaluation and educational innovation. In addition, it provided an opportunity to see a rural setting akin to where Geoffrey's youngsters, "poor rural white children" had come from. All this in the background of a computer assisted instruction program in mathematics. The study ended up as a problem in community settings and interorganizational difficulties in innovation. As the events occurred in the setting, several important organizational aspects arose: we were allowed to follow the events, treat them as problematic, and eventually publish our descriptive/analytic account of the "new" problem. Usefulness to the organization seemed to be an early blend of initial congruence in program needs and problem statement and a later blend of the reports and dialogue being of some use to the program staff and being well received by one or another of the subcommunities within education--AERA, Council on Anthropology and Education, or more specialized groups of evaluators, OE/NIE staff, or prestigious individuals around the country.

The General Reinforcement Package Project, the Observations of the Comprehensive School Mathematics Program and the Extended Pilot Trials of the AEP, a qualitative, description, analysis and evaluation all had these characteristics. For instance, I found no difficulty in getting caught up in a Skinnerian, behavior modification, social exchange kind of program. This was a point of view I had found only partly applicable in Complexities; now was a chance to see it full scale under ideal conditions. Further it provided a return to the inner city; this time to a Black ghetto school. It was also a community in which Bryce Hudgins and I had worked many years before. The problem was phrased this way.

While our work in the General Reinforcement Package Project (GRP) has not begun as yet, we anticipate several possibilities regarding the descriptive and theoretical results of our evaluation efforts. Phrased most succinctly, we see the major thrust as an 'alternative conceptualization' of the program. Obviously, we are not arguing that any one theory is better than another in some simple overall way. Merely, we are stating that our work in comparable kinds of urban classrooms has utilized different models and conceptualizations, that is, social psychological theory rather than reinforcement, behavior modification or social exchange conceptions. Phrasing the realities in a new set of terms presumably will have utility for adaptors as well as skeptics.

The more important thrust of the effort, however, lies in the potency of the 'clash of theories.' Our hope would be to face the original theory with new data and interpretations which will then facilitate extensions in the original theory. If these extensions can be translated into new practical procedures, the nature of the program per se will be enhanced. For example, our brief conversations with staff from GRP suggest little attention to social roles played by children in the classroom. Our earlier work (Smith & Geoffrey, 1968) suggests that this is an important aspect of classroom social structure. The children who were jesters, troublesome, or on 'no work' agreements were quite important in the ebb and flow of interaction and instruction. What the exchange program does to such phenomena and what then are the further consequences seem to be reasonable illustrations of such a 'clash' of theories. (Smith & Carpenter 1972)

The generalization evolving from these studies is that making things problematic becomes easier as one's programmatic effort increases in scope and differentiation. As settings are linked, as substantive issues are opened up, as nagging problems from previous studies, i.e., "more research is needed" then only a little thought is necessary to see "a problem" or build a rationale for its importance. Now it seems, I can fall into almost any educational setting and generate five issues worth working on.

Through all this another generalization regarding the origins and evolution of inquiry problems focuses on the dilemma of maintaining one's intellectual integrity versus doing something useful for the organization. In general the dilemmas got resolved with productive syntheses; however in one case I gave up an important problem I really wanted to do but ended up in an equally intriguing one.

The problem about which I couldn't convince several key CEMREL staff members was what I was calling "the degree of implementation problem." It is a major issue in conceptualizing and evaluating curriculum innovations. It had come up in the Rural Highlands study, the CSMP study and the ISP study. I thought we might mount an attack with a qualitative-quantitative strand, a theoretical strand, and an empirical strand. Not doing that was a major disappointment then--and now. However, that year I got involved in the first of several studies in the Aesthetic Education Program. The conflict here, as with many of the choices tends to be an adient-adient one, the "penny candy counter problem," how to decide among two or more desirable alternatives. Not only is that an interesting problem in the origin and carrying out of field study's problems, but it seems crucial to a theory of the origins of field research problems.

Aesthetic Education: In some ways writing about the area of aesthetic

education is most difficult, for here I had some high hopes, ran into a complex set of conflicts, and finally felt the only way to maintain some kind of personal and intellectual integrity was to resign from CEMREL. But that's also another story. The first project was an observational description, analysis, and evaluation of the Extended Pilot Trials of the Aesthetic Education Program. Sally Schumacher and I spent a year coming to grips with a statewide dissemination effort. The problem originated in organizational needs and in my continuing desire to extend the scope of field methods.

Several paragraphs from a brief initial memo state "the problem."

This memo attempts to state in a preliminary manner, our efforts in evaluation in the AEP Middle State project during 1971-72. At this time there seems to be two central problems:

- 1) a description, analysis and critique of the extended pilot efforts to establish the AEP program in Middle State.
- 2) the development of a five year evaluation plan (Component #5 in the undated, unauthored document entitled "Pilot Aesthetic Education Program: Middle State") for the larger project.

The conclusion of the memo focused back on the audience, outcomes, and possible uses of the document.

Two critical interrelated problems have been identified as major issues in the 1971-72 evaluation effort of AEP's Middle State pilot project. The description and analysis will culminate in a document hopefully useful to personnel in the State Department of Education, in the cooperating schools, and CEMREL. If our past experience holds true a number of substantive issues in diffusion strategies, psychology of teaching, and pupil behavior and experience will arise and these will

be of more general interest to the educational community. Also, as a sub instance of the above concerns, a number of problems and issues will be identified, collated and formed into a five year plan of evaluation.

In our final report we commented about the antecedents and the functions of formalizing an initial statement of the problem.

One of the happy consequences of "bureaucratic science"² is the demand for initial problem statement, methodological rationales, and anticipated formats. This demand seems particularly functional for evaluators using qualitative observational procedures for it sets the limits and scope of the observational research thrust. Also, when one begins to record and interpret interesting and somewhat anomalous findings, the early statement of the problem helps in defining the major and minor themes in the data. That is, it helps answer questions such as, "Why is this important, what does that mean, why do I keep thinking about that issue?" In addition, these original problem statements have positive consequences in defending oneself from changing agendas "Why didn't you do Y and Z?" The response to this is that "We agreed to X, as indicated in the original documents." (1972, p. 1)

Gradually I began to see the aesthetic education area as a prototype case for all the knotty problems in a general theory of education. If the 'problems' of priorities, of creativity, of expressiveness, of measuring learning outcomes could be solved then a framework for attacking any area of education would be open for analysis. The domain seemed rich enough,

² Interestingly, Mills' (1959) analysis of "bureaucratic science" tends to accent dysfunctions rather than functions.

complex enough, open enough for such a major effort. A couple of pieces developed in that direction (Smith 1975, 1977). Mostly now, I have a set of partially finished manuscripts, a set of first draft proposals and problem statements, and a large set of frustrations. Mixed in however is a kind of quiet optimism that sometime, somewhere I'm going to get back to them. Too much of my world has played back on itself to doubt that this will happen also. If we need a large concluding generalization it's this: there's a large body of foreshadowed problems, in the best sense of Malinowski's term, waiting to be explored, developed, analyzed and integrated.

2.3 The Transitional New Zealand Year

Immersed in the Aesthetic Education period was a sabbatical/Fulbright year at Massey University in New Zealand. That experience turned out to have far reaching consequences on "the problems" I was working on.

I had gone to New Zealand to work with Ray Adams on the problems of qualitative/quantitative observation with video tapes. We have ten hours of tapes, of five class periods teaching education students about the AEP program, which he helped me record. Although we taught an exciting joint seminar, I found I couldn't stay with that qualitative/quantitative problem that runs back through Teacher Awareness and Go, Bug, Go. The issue that caught me was the Hirst/O'Connor/Struthers debate on the nature of a theory of education. That one has both ordered and fouled up my professional life ever since. It's refocused totally "the problem" I'm working on. At a very abstract level it now is the major justification if not source of every "specific problem" I'm inquiring into. It's transformed the research serial from a series of case studies to an organized structure.

For instance, "Mrs. K's Drawing Class" has been through two major

drafts, once while in New Zealand and once for a colloquium at Michigan State. It still isn't finished. The genesis of it as a problem was simple--my wife and I were exploring some adult education areas after our children were grown and gone, and I wanted some first hand direct experience in an art form to add that kind of tacit knowledge to the Aesthetic Education Program in which I was working. So we enrolled in a 10 week drawing course in a local night school program. It proved to be interesting on several counts, I began to take notes, and pretty soon I was into a project. The "problem" that evolved was looking at the classroom from the perspective of a student. While on sabbatical at Massey University I decided to do a quick and dirty write-up of the notes. The data were limited; I'd have a first specific product for the year; the schedule I was on was reasonably leisurely. Then the problem evolved once again. A number of my colleagues at Massey were into educational philosophy and theory, particularly as expounded by the London University group. Soon I was over my head in Peters, Hirst, and Dearden and into the Hirst/O'Connor/Struthers debate on the nature of educational theory. Poor old Mrs. K's Drawing Class evolved into the concrete vehicle for understanding that debate.

Neither it nor I could measure up.

That exercise, plus the indigenous New Zealand culture spawned several other activities. My wife and I "got into" spinning and weaving. I also took a course in Maori wood carving. It was Mrs. K all over again, "Teaching and learning in three art forms: a student's perspective." More notes, a variety of interpretive asides, and an unfinished project.

In addition, I have several note books on 10 days on a Maori Marae with a group of faculty and students from a New Zealand teachers college. We were learning Maoritanga, the culture of the Polynesian New Zealanders,

visiting schools, and learning something of Maori crafts--weaving and carving. In addition we spent some time on another Marae learning Tukutuku paneling and participating in opening day ceremonies. Old themes from the arts, rural education, teacher education, and new ones about multi-cultural societies, their conflicts, problems, and possibilities run all through these notes. Neither the occasion, the time, nor the resources for doing something with these data have arisen as yet. But they're there, and I'm patiently confident the occasion will come along. In a sense, what was once simply a research method later evolved into a broad flexible methodology in tackling a variety of substantive problems, and now had become a life style pattern for processing--perceiving, reflecting and evaluating--experience in its most general sense. Whether that's a disease, obsession, or some kind of virtue I'm not sure. The phenomenon is important, so I believe.

2.4 The Recent Years: Toward a General Theory of Education

Figure one shows several problems in the last few years whose origination and evolution need at least brief comment. Here, too, a context exists. The New Zealand experience focused a number of the methodological issues into a possible problem--a book of essays on methodological issues in participant observation. I thought it was an exciting idea and a way to bring a variety of ideas, many of which were unpublished AERA presentations and partially developed ideas, into focus. Three events happened. Several book editors I talked to informally were not encouraging about anything but textbooks. I was very busy and was having a difficult time moving the project along. And thirdly, a proposal to get NIE funding was turned down. A bit discouraging to say the least. At AERA, Lee Shulman and

Larry Iannaccone of the AERA publications committee asked me if I would do a methodology chapter for RRE. In deciding to do it, it seemed an opportunity to say some of the things I had on my mind, to write an introductory essay which might better state the case for the proposed book of essays than I had been able to do up to that point, and generally get the issues out of my systems so I could go on to something else.

That took on an autobiographical case study format, grew to twice its original intended length, received some initial encouraging words from friendly reviewer critics, and finally settled, for the moment, some long term problems in a theory of methodology. The analysis of issues in 1) quality of data, 2) the descriptive narrative, 3) the kind of theory and 4) metatheoretical dilemmas were problems that Geoffrey, Keith, Kleine, Connor, Pohland, Carpenter, Schumacher and I had wrestled with for a decade and a half.

Those discussions began while I was in the middle of Science education in the Alte Public Schools: A kind of case study (Smith, 1977, 1978), the large multiple case studies in science education project, directed by Bob Stake and Jack Easley. In one sense, they defined the problem; in another sense each of the site researchers defined his/her own agenda. Mostly I was interested in conceptualizing curriculum at the district level, beginning to develop a historical dimension to our analyses, and furthering some of those unfinished ideas in science teaching that Brock and I were still puzzling over. At another level, the problem I was solving was getting myself out of the doldrums which had set in after the CEMREL resignation. The latter may have been the most critical "problem" under investigation.

The RRE essay on methodology had hardly been drafted and revised when

David Dwyer and I began a study Improving Urban Education: Federal Policy in Action. In this we began to run into issues in studying "one's peers, if not one's betters." The methodological appendix to that is on its way and has a strong ethical thrust. The procedural actions in field research are never simple. Always there seem to be pro's and con's for every step. Entertaining those, reflecting upon them makes them problematic for a theory of methodology. Seeing them in their human context gives them a quality of drama.

The substantive problem that Dwyer and I are working on had grown out of a complex interaction from consulting on the Inquiry and Assistance Project (IAP), from my belief that the project itself was a microcosm of urban education issues, and from the conviction of the Federal Education Department that "a history and analysis of IAP" might be an important contribution. In effect we are trying to understand the genesis of that project in the federal bureaucracy, the mechanisms of its selection and development, and its working out in action.

Finally, I need to mention the origin of the project, Kensington Revisited: A 15 Year Follow-up of an Innovative School. It characterizes perhaps best of all the combination of happy accidents, serendipities and making the common place problematic, if not dramatic. It began simply. Matt Miles called and inquired about being on an AERA panel, Case Studies of Innovative Schools. He was hoping to have two "classical" studies, ours and Neal Gross' Cambire case and two new studies, one by Michael Fullan and one from his current project. I was supposed to speak about a re-consideration of key explanatory variables in our earlier analysis of Anatomy. In a way I felt kind of stupid. I hadn't been back to the Kensington School in almost 10 years; I'd seen only a few of the original

faculty over the years. I considered a quick and dirty return to the school or a brief questionnaire and perhaps some telephone interviews. None of those alternatives sat very well. I'd been involved in "pot boilers" before and mostly they spelled trouble. I finally decided to argue some initial ideas about the general theory of education problems that had been brewing. While these musings were going on I received a copy of NIE's Organizational Processes RFP. Immediately upon scanning that, the possibilities clicked regarding a follow up study. The initial "problem statement" went through several drafts, several discussions with the current superintendent of Milford and an old friend or two from Kensington whom I'd kept up with professionally over the years.

The problem statement on that occurs at several levels of abstraction and several degrees of specificity. From a common sense perspective we hope to find out what's happened to the school as an educational organization and what's happened to the faculty. Without laboring the technical problems they include middle level theories of organizational structure and process, personality theory and organizational change, an educational test case of cultural, organizational, and social psychological change theories and finally a continuing move toward a general theory of education. Each of those differentiates into a cluster of subproblems. But all that can await another time.

3. IDEAS TOWARD A THEORY OF THE FIELD RESEARCH PROBLEM

While a variety of lessons, morals, or interim generalizations might be drawn from a case history such as this, it is the origin and evolution of problems which I wish to focus upon. This section should really be an analytic essay on "problem solving about problem origins." That kind of theory will have to wait for more time and space. For the moment I'd like to collate a few reasonably simple statements explicit and implicit in the narrative. These will be organized in two clusters, the concept of problem per se and the antecedents of the field study problem.

3.1 The Concept of Problem

Unpacking the Label: In reflecting upon the narrative, the very definition or concept of problem has gradually moved toward clarity. Earlier statements from a more behavioral or measurement point of view in educational psychology never sat well with me, although buried in his NSSE chapter Robert Thorndike (1950) caught the essence of a conception I found workable.

"Any problematic situation involves, in the first place, an objective-- a final end result to be achieved; it involves, in the second place a set of given facts and conditions; it involves, finally, a gap between the given and the desired, which must be bridged." (1950, p. 198)

Now, I believe a problem is a second order relational concept, like relevance, as Meehl (1972) analyzes that term. In brief, a problem is a discrepancy between where one is and where one wants to be. More formally, a problem requires 1) a statement of a goal, an end in view, a preferred or desired state 2) a statement of a present position, condition, or state and 3) a statement of a discrepancy between the desired state and the present

state. Most of us seldom carefully articulate for ourselves those first two statements, where we are and where we want to be, and, hence, we often take over someone else's vague belief about presumed gaps or some average statement of gaps possessed by a number of individuals; as in "problems" in an arithmetic test. Only when I poke around within the single case does the meaning of the label "problem" take on its full dimensions. Every problem in Figure 1 had a specific concrete thrust of its own, a particular gap that needed closing. That discrepancy and its resolution was always important enough, at least in my eyes, to stand alone.

Problems Encased in other Problems: In the narrative the comment occurs "that problems are encased in larger problems and that a reconstrual process is going on constantly." Those two points require additional comments of several parts. Analytically they are separate issues; practically they often occur together. When Geoffrey asked me to come observe his class, the "problem" was any one of several:

- 1) Produce an educational case study comparable to the half dozen contained in George Homans' The Human Group. The desired state, the present state and the discrepancy are clear.
- 2) Figure out how a middle class teacher copes with a group of lower class youngsters. It was clear to me that it would be important to know this; I didn't have any idea of how the coping occurred; the discrepancy was self-evident.
- 3) Develop a theory of teaching. We were beginning to feel the importance of a theory of teaching. Our view was that the present state of theories of teaching were limited.

The field study problem, the observation and analysis of Geoffrey's classroom

could be placed in any one (or all?) of the "larger" problems. By larger I mean more abstract, more general, or more comprehensive set of conceptual frameworks--statements of desired positions, current positions, and discrepancies.

The Evolution of the Problem: The case study problem seems to evolve in at least three ways: within the specific piece of research, within the more abstract set of concepts, and, overtime, in the larger research serial. Specifically, if we focus on the statements of the desired state regarding our field studies this has changed very dramatically over the years. For instance, initially I tended to phrase the desired state as:

1. a particular substantive goal--e.g., coping with lower socio-economic status kids.
2. a piece in a series of case studies of schools, organizational positions, curricula, etc.
3. a part of a larger, more comprehensive, and more abstract structure, a general theory of education.

That third level of construing the problem was one of the major outcomes of the Fulbright year. Conceptually, freedom, time, stimulation by bright young philosophically inclined social scientists, seemed to have been the critical elements. If one looks to consequences, the most significant seems to be broadening the base of the legitimacy of specific problems.

In an important sense, anything that happens in or about schools and other educational settings, organizations, and institutions now qualify as steps on the road toward closing gaps between the present state of affairs and this very large desired state. From another perspective it makes rationalization easier.

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The Pragmatics of Theory and Problem Definition. Early on, as in the discussion of the origins of Complexities, the problem, if it began as a practical concern, was usually then phrased alternatively as a problem in educational theory and as a problem in social science theory. The latter was sometimes psychological, sociological or social psychological. In retrospect that seems very important. "The" problem seems always to have been any one of two, three or four problems. My guess is that that sensitizes one to different nuances in the reality one observes and that it helps to break open the problem in unusual ways. Perhaps this is just an alternative way of speaking of flexibility, changing directions, and lack of an overriding Einstellung.

People seem to treat theory in different ways. To some, its structure is its essence. Elegance of form, the clear definition of mutually exclusive parts, coherence, internal consistency are the criteria of its quality. To others, the central criterion, not unrelated, but of first priority, is the functional, pragmatic quality of the theory: will it help solve the problems one is curious about. That's a harsh razor. And it's one that changes, somewhat, as each new problem comes along. For this discussion, the important point lies in the redefinition of what the problem is and the residue which is left for construing the next problem. More and more that seems to me to be the central criterion.

An interesting corollary of the pragmatics of theory exists. Perhaps it's better described as a mechanism. It involves the reconstrual of practical problems, the kind superintendents, principals, or teachers raise in "What would you do about X?" or "What do you think about Y?" The trick, the ploy, the mechanism involves the translation of the problem at that level into one or more of the theoretical systems that one tends to work in.

Translation may be the wrong label, reconstrual may be closer. Translation involves a more literal one for one shift, as in translating a passage of German into English or vice versa. Here, the literal translation occurs but there seems to always be a little more, a reconstrual, for the theoretical language always has its own built-in assumptions, perspectives, and related concepts and propositions which change the problem somewhat as soon as the translation is underway. That is, it does if the theory has any potency in its own right. In the new language system the problem is partially redefined in the translation. Multiple translations give multiple partial redefinitions and reformulations. Then one works on the problem, conceptually and usually empirically, that is one goes out and observes, and "solves" it in one's own terms. Later, one translates back to the language of the party who originally posed the question. Gouldner's (1961) essay on theoretical requirements of applied social science triggered off most of this originally.

Conclusion: Now the complexity of the concept of field study problem can be synthesized along the lines of Figure 3, a concatenation of elements.

Insert Figure 3 about here

The overall concept analyzes into three elements: a preferred state, a present state, and the discrepancy or gap between the preferred and present states. Each of these is analyzable into several components. For simplicity of illustrative purposes the figure indicates the elements within the present state. The present state has a structure, a history and a context. In turn each of these elements, for example, the structure, can be considered

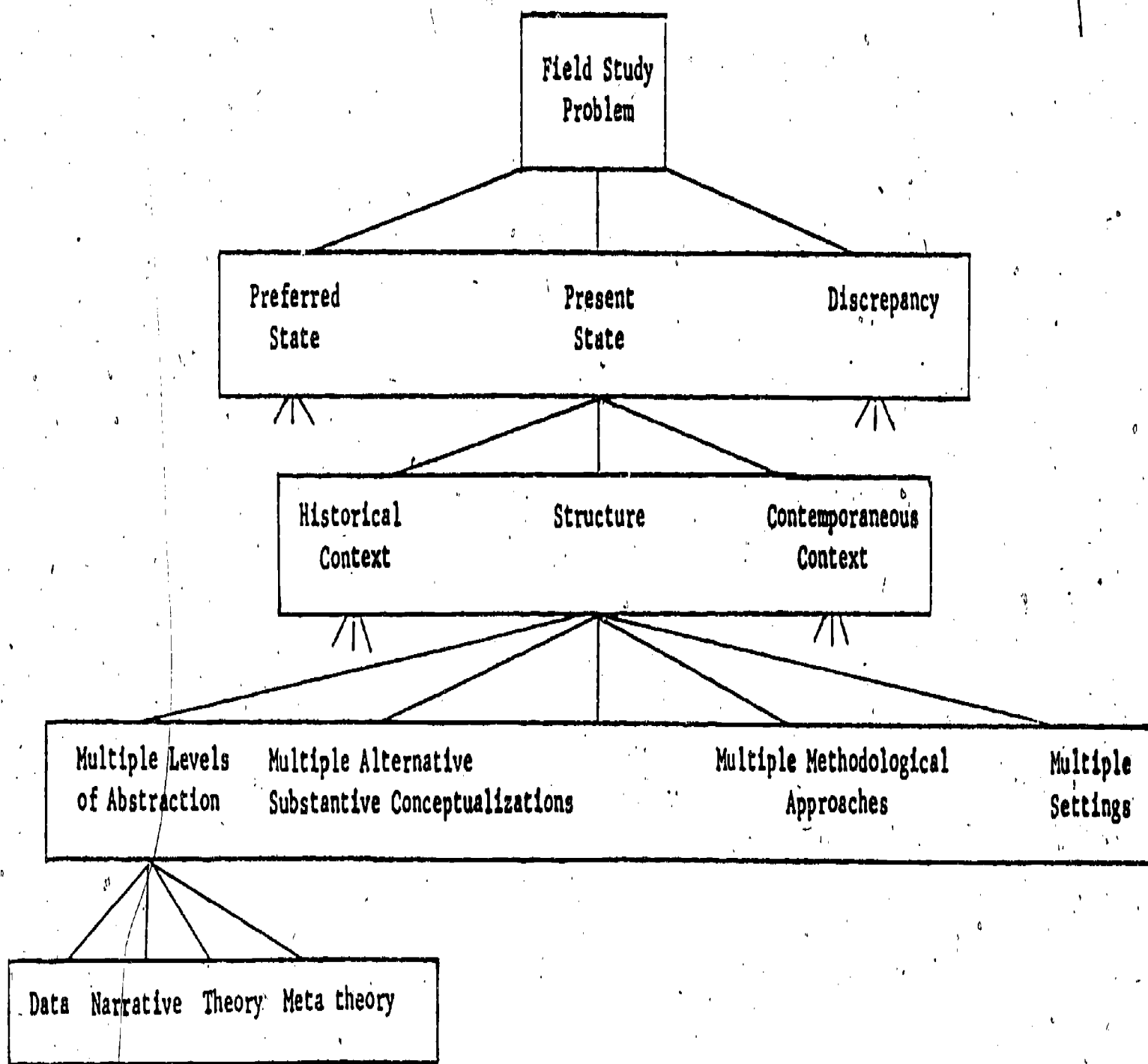


Figure 3 The field study problem as a concatenation of elements.

at multiple levels of abstraction, can be conceptualized in terms of multiple alternative substantive theories, with multiple methodological and procedural wrinkles, and in multiple concrete settings.

At different points in the narrative, the field study "problem" seems to have arisen, shifted and evolved in one or another of these multiple ways, that is at one or more points in the concatenated pattern of elements in Figure 3. Something as complicated as this seems necessary to handle the problems of the several field studies.

3.2 The Antecedents of the Field Study Problem

For the moment, I would like to return to the simpler, unanalyzed label "problem" and to a consideration of the antecedents of the field study problem. Figure 4 presents these as an inventory of determinants.

Insert Figure 4 about here

If we follow Webster, an accident is a chance or improbable event. Open systems, such as human personalities, even in the form of educational inquirers, are impinged upon by accidents, improbable and chance events-- Geoffrey in a summer school class, the Milford School District building our innovatively designed building, an RFP being issued at a particular time. An anomaly is an irregularity, an item out of place or out of order. Implicit in that definition is a known regularity or order and someone who perceives the out-of-placeness of an event. The apprentice at the Washington School wasn't an anomaly to anyone but me. Anomaly is another of those second order or relational concepts which are difficult to handle empirically. Serendipity is finding valuable things not sought, sort of a happy accident,

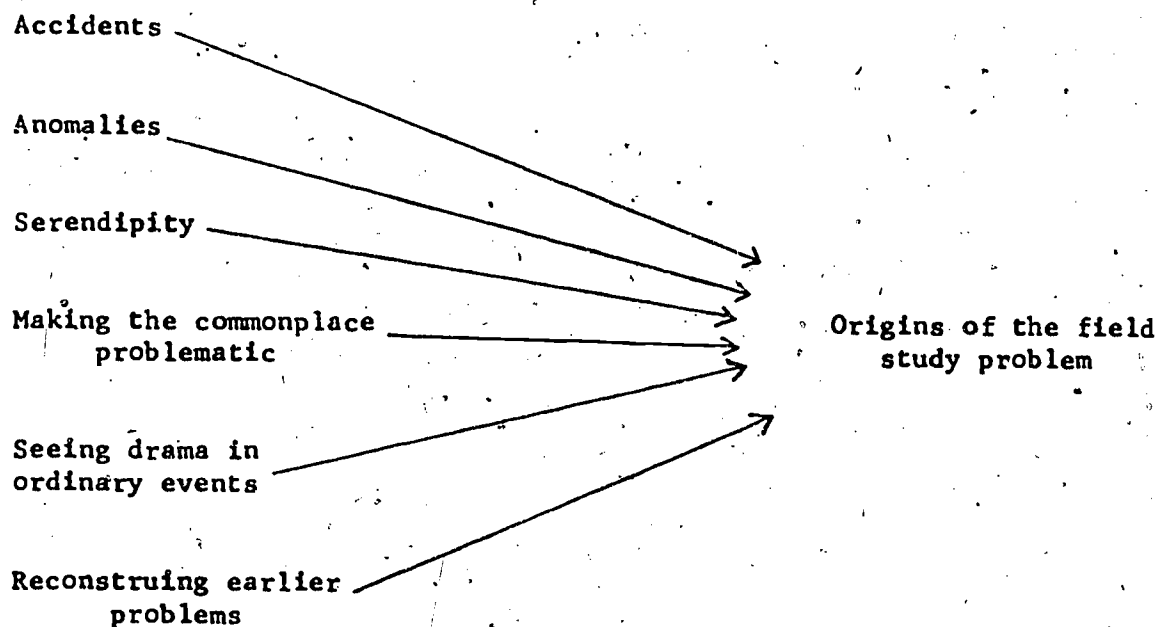


Figure 4 Antecedents of the field study problem

or an anomaly with an attached value. Being a part of the early history of CEMREL is an important instance. Making the commonplace problematic seems to be a knack for seeing questions in the mundane or unsettling and making unclear the obvious or the usual. Converting earlier problems into relevant topics for conferences and symposia seems to generate new problems. Seeing drama in ordinary events is one definition of aesthetic sensitivity. More colloquially, it's "making a big deal" out of ordinary experience. Or, in Dewey's (1933) terms turning everyday experience into "an experience" that is isolating, bounding, and labeling a portion of the unending flow of experience into units or episodes which have a beginning, a middle, and an end. It involves giving experience structure. "An experience" escalates into "memorable experiences" and "peak experiences" as the value import increases (Broudy 1972, Maslow 1959, Smith & Schumacher 1972). Finally, a more purposive planned kind of source of new problems appears in the more deliberate attempts to reconstrue old problems. Working that way turned an aesthetic education program evaluation into an analysis of the teaching effectiveness (Smith 1977) and a position on interorganizational structures as educational networks (Smith, in press).

With these more general labels in mind, it seems reasonable to return to more explicit specification of the antecedents to the problems raised in the narrative, sort of the antecedents of the antecedents. Such specification moves one toward more action implications, a number of which seem potentially modifiable.

3.3 The Inquiring Field Worker

The Non Tabula Rasa: Invariably the complex field settings we have studied have never been entered tabula rasa. Our curiosity and perplexity

always seems to outrun our answers or solutions. We always seem to have multiple phrasings of our central theme or hunch--some common sense, some practical, some from one substantive or disciplinary thrust and some from an alternative. Education, psychology, sociology are so full of schools, favored constructs, fads and fancies that only a little effort is required to set up multiple and usefully conflicting images, models, concatenations or predictive systems. As these are arrayed against the flow of events, they are clarified and help clarify. More formally these can be phrased as foreshadowed problems. Even more formally they become competing theories (Smith 1979).

Conviction about Non-Conviction: Coupled with the "non tabula rasa" is a conviction about non-conviction. I find myself out of step with true believers and men and women who are sure of themselves, their perspectives, and their points of view. As functional as those might be for charisma, for leadership, and for some kinds of teaching, they seem less functional for generating problems. When one isn't sure, when one isn't convinced, and when one has doubts every action becomes problematic. The trade off seems to be giving up such benefits as certainty and its aura for the benefits of doubt and problematics.

Personality Processes: Aesthetic Sensitivity: Intellectual and affective dichotomies do an injustice to the origin of problems. A lingering and perhaps most important set of hunches, is summed up best by the phrase - making the common place problematic. In my more expansive modes I think this is akin to Harry Broudy's conception of aesthetic sensitivity, the ability to see drama in ordinary human events.

To experience life aesthetically is to sense the drama in every event

of nature, in every moment of life, the conflict of colors and shapes, sounds and rhythms. To go beyond stereotyped dramatics requires cultivation, as it does to go beyond stereotypes in any other type of experience.

The artist differs from the non artist in being able to give form to sensory materials or to see form in them that most of us do not. This is disconcerting to the non artist because for him, dramatic form to be discernible has to be writ larger in plots and conspiracies, in great misfortunes and disasters. Artists find dramatic form everywhere. (1972, p. 37)

On several occasions we have tried to take seriously Broudy's almost poetic conceptions and relate them to empirical events (Smith & Schumacher 1972, Smith 1974, 1975, 1977).

Figure 5 represents an attempt to develop a set of hypotheses beginning with his conception of aesthetic education. He argues a perceptual approach which is contrasted with a performance approach and an appreciation approach. Personality theorists, especially of a trait persuasion might argue with the generality of "aesthetic sensitivity," across the arts much less in scientific domains. Similarly the ability (or abilities?) to sense the dramatic might be difficult to separate from the ability (abilities?) to make the common place problematic. And so the hypotheses continue into the making of the productive field researcher. The array of researchable problems all this suggests seems enormous. Must one select students on alternative criteria? What are the broad and narrow curricular issues in training? What happens to the traditional course in the introduction to educational research methods? How does all this relate to our earlier concepts of practice based inquiry and research serials?

Insert Figure 5 about here

The Research Serial: These several points culminate in the need for a concept such as serial. As Murray and Kluckhohn use the term:

"Many actions, though temporarily discrete, are by no means functionally discrete, they are continuations of a shorter or longer series of preceding actions and are performed in the expectation of further actions of a similar sort in the future." (1954, p. 10)

If one's research activities are of the nature of a long term serial, as I would hypothesize, from the case, then theory and research on field study problems must confront the issues at that level. Do researchers perceive and talk about their work this way? Do young graduate students have any conception of research serials? Do research training programs err in focusing on the dissertation problem rather than on the serial which is then differentiated into specific problems, one (or maybe several?) of which becomes the dissertation? Do university faculties have norms related to research serials? Do funding agencies have such a concept in their perspectives?

The problems now always seem to have a connectedness or relatedness to earlier efforts. Perhaps that's a facility for rationalization, a trait of which I've been accused on occasion. That connectedness is open ended and evolving, as in the shift from looking at teaching acts per se to doing a shelf of case studies of educational settings, to a general theory of education. On occasion it is very specific, as in wanting an intellectual substantive record in Pat Brock's class, and on occasion quite general, as in testing the limits of the methodology in the

Rural Highlands project. There seems to be a complex interplay among:

- 1) substantive themes - classrooms, teaching, curriculum, organizations and policy,
- 2) methodological issues - new settings, testing limits,
- 3) funding possibilities - OE, CEMREL, Fulbright, NSF, NIE,
- 4) organizational press - heavy teaching, mutual adaptation, collegial possibilities,
- 5) dissemination and publication possibilities.

3.4 The Social Context of Field Work

It doesn't take many experiences of having the same research proposals turned down and later accepted and having the same manuscripts criticized and lauded in the pre and post publication process to realize that opinions, values, and group norms have a good bit to do with the practice of social science. And if one believes with Kuhn (1962) that paradigms are important in the sociology of science and with Lodge (1976) that paradigms are ideologies, the point is generalized even more broadly. Such experience and reasoning can leave one jaundiced and remorseful that the life of science isn't as pristine as some of us once believed or some of us still preach. Or it can leave one with a need to reconceptualize science more "realistically" and use that "better" theory when one thinks about the genesis and evolution of the "problem" in field studies.

Resources and Problem Definition: Beyond the intellectual origins of problems, a number of social and organizational factors seem to have been critical in defining the context for selecting problems to work on. For instance we have a 5 course/year teaching load at Washington University; for me this is too heavy to maintain a full research program. Consequently I've tried to find half time support for activities. If two problems were

equally appealing, and only one had support then I'd go with the dollars. The difficult problem is the cut off point or intersect of declining problem interest and availability of resources, that is, when do you sell your soul? A variety of occasions have occurred when I've said easily, "that's too far afield." There have been a couple of occasions when it's been more wrenching to turn and walk away. The more usual instance has been the attempt to take a problem we've wanted to work on and to creatively build integrative resolutions which integrate with RFP's and organizational mandates. The relationship with CEMREL typified this kind of action.

Collegiality of Educationists: Similarly, my colleagues and I often complain of too many varied demands--teaching undergraduates in elementary and secondary teacher training, teaching M.A. students in curriculum, improvement of instruction, administration, counseling and educational psychology and teaching similar varieties of Ph.D. students. However, these students directly and indirectly (if one tries to be even a bit contingent) has given our work a continuing push toward application, practice, and utility of a particular sort. Time after time I've found myself in class being faced with tough and difficult but relevant questions by experienced teachers. Later, these same questions are on my mind as I enter settings such as the Washington School, the Kensington School or the Alte School District. How do these educators answer those questions in their day to day action, in their own language systems, and in the building or district norms and ideology. And this is different, I think, from being in a more academic psychology or sociology department or being full time in an R & D Center.

The G.I.E. as a Social System: And then there's the array of social

system antecedents that lie in the norms of the department and university to which one is attached, the kind of interaction that that spawns, and its interdependency with leadership patterns in those organizations. All this requires an essay on the history of the Graduate Institute of Education, which in itself is an interesting story, and for some of us, a very important story as well. Suffice it to say that I've been fortunate to have had over the years a number of able colleagues from several disciplines, with quite varied experience, alert to an array of important issues in social science and education. They are supportive, critical, challenging and interested. We play all sorts of games, such as "Have you seen X?" or "Do you know Y?" to which you're supposed to counter "Yes, I think it's related to, or said better by Z." Most recently, Arthur Wirth stopped by, as I was writing madly toward a project report deadline, and quoted a line or two from Janik and Toulmin's Wittgenstein's Vienna on the belief that the really important items are ineffable and can't be written about. The double thrust rested in his knowledge that I'd been reading and making a bit of a to-do about Toulmin's Human Understanding and his several books on the history of science. My earlier thrust at him had come back in a friendly riposte. Now, halfway through Wittgenstein's Vienna, I've found a model which has crystallized a more vaguely stated next problem which has been evolving for six months. That kind of collegueship has existed at Washington University for me for almost twenty five years, an incredible happy accident.

Dissemination: Conferences and Publications: Another intriguing aspect of having a broad front of research activities is the "What do you do with all of it?" Some of the defining and redefining of problems occurs

in the dissemination and publication outlets. In a sense I'm always on the outlook for initiating or responding (counter-initiating) to people who ask me to be on AERA symposia, to attend conferences, or to write book chapters. If their theme, as in the present conference on the study of schooling, is open enough to capture something I've been involved in, then I find that a "problem" is generated which permits reworking or extending earlier data and interpretations. For example, the Monterrey Conference on Qualitative and Quantitative Research provided the opportunity to talk about Teaching Effectiveness. The several projects in Aesthetic Education which had been only in technical reports could be adapted and extended. Each time the problem changes--sometimes just slightly, other times quite significantly. Conference themes, participants and audiences pull one in varying directions and in varying amounts. Sometimes it makes for creativity.

3.5 Research Methodology and Procedures

As I look back over the narrative several issues arise and cluster around the rubric "research methodology and procedure" in the genesis of field study problems.

The Importance of Educational Settings: Most problems are closely linked with complex practical settings where it is obvious that educational events are underway--smoothly or roughly, sterily, traditionally or innovatively, consciously or unconsciously, dully or enthusiastically and so forth. Teasing these apart is the essence of field research, and illustrates the power of the methodology. One of George Homan's wise analogies has stayed with me. He comments:

The final emphasis must always be on the group before us. Lord Nelson,

greatest of all admirals, after explaining to his ship captains the plan of attack he intended to use at the battle of Trapalgar, went on to say "No captain can do very wrong who places his ship alongside that of the enemy." In the same way, no one who studies a group will go far wrong if he gets close to it and, by whatever methods are available, observes all that he can. Nothing that can illuminate the group should be ruled out for doctrinaire reasons. We shall be blind enough without wilfully narrowing our vision. (1950, p. 22)

If we change "group" to "educational event," we have begun to extend the concept of practice oriented inquiry. And we have begun to see more clearly the origins of field research problems. Whether one ultimately takes a Peter's (1965) initiation into worthwhile activities or a Dewey (1934) "reconstruction of experience" as the central definition of education, beginning one's study in settings where there is common agreement that the events are educational means that everything one trips over is an educational problem.

Until recently, except for a few sports such as Barker, Gump, Kounin and their colleagues in psychological ecology, psychologists have not been greatly interested in "settings." In Education, a self evident importance exists. If a high school class carries the label Algebra I and another is called Senior English, some very predictable and important differences in verbal behavior will be occurring. Methodologically, we seem to be vacillating or alternating between "settings looking for a problem and problems looking for a setting." Half the time we seem to have an issue, a hunch, or a question which demands a setting to observe wherein it will have half a chance of a fair trial to see it working out. The Kensington School attracted us because it looked like a place to see

the genesis of a faculty peer group.. In turn, once there, a host of things happened that reflected a setting looking for a problem. As we talk with students, the advice often involves--observe, tell the story, and then look to its meaning or explanation.

Inquiry Procedures as Implicit Social Science Theory: Social science by the very nature of its subject matter--social action of individuals, dyads, groups and larger collectivities--has a different relation between its theories and its methods than the biological and physical sciences. Doing social science is a kind of social action. In a sense, social science is one item of social action working on another item of social action. In Complexities, we invented--fell into--the inside/outside points of view, being there all day every day for a semester, and collegial authorship. These common sense procedures implicitly reflect major theoretical and meta theoretical issues--multiple perspectives, inner and outer perspectives, concern for process, unity of social events, and social science as a collegial enterprise. As we have reflected on these (e.g., Smith 1979, Smith & Dwyer 1979) we have used words like reflexive and interactive to capture the play back and forth between the language or theory of methodology and the language or theory of the substantive issues. Our conviction now is that the same language must be used for both of these kinds of social action./ The common sense procedures that seemed reasonable at the time in solving methodological questions significantly affected the data that was generated and the subsequent theoretical analyses and interpretations that were constructed. At times we seemed to do better than we knew. In hindsight we've sought to build rationales potent enough to handle the procedures and the substance in our framework.

In conclusion, at its ultimate, I believe that the concepts used to talk about social science methodology, that is one's theory of methodology, should contain the same concepts as one's substantive social science theory. That is, a reflexive quality should exist between substance and method. At that level, or in that form, one's field study problem looks to have neither beginning nor end, which is a nice sort of a dilemma.

3.6 Conclusion: Multiple Origins

The problems came about in a number of ways, and this suggests being wary of any simple, facile generalizations about the source of field study problems. They arose in questions from students, from comments by faculty colleagues, from anomalies in settings, and from persistent internal naggings over intellectual loose ends that need to be tidied up. Some seemed to be accidental, some serendipitous and others from making the common place problematic or dramatic. As we have reflected upon this one of our hunches is that the methodology has a fit, a congruence with the common sense, practical questions teachers raise--coping with urban kids, beginning an innovative school. The data, the narratives, the concepts and miniature theories seem helpful to them in thinking about their problems in their role as responsible professionals. It may be that the problems have been there but that more classical experimental or quasi experimental designs, multiple regression analyses, or surveys could not easily handle questions of action and practice at that mundane level of social action. If there is something special about field methods in this regard, then it may be bigger than and more important than helping clarify problems.

4. SUMMARY AND CONCLUSION

This started out to be a brief story on some hunches about the origins of field research problems. In the course of writing it became a case study of one research program. As such it has all the hazards of the single case and some of the benefits. In telling the story, a number of tentative generalizations were suggested about the origins and evolution of field research problems. Most of this was tentatively summarized in the title "accidents, anomalies, serendipity, and making the common place problematic." At a slightly more abstract and theoretical level, practice based inquiry, research serials, aesthetic sensitivity, and educational social systems, seem legitimate foci of interpretation. As these ideas developed I had hoped to compare and contrast with several other statements which in one way or another have served as models--both tacitly and explicitly for our work. Skinner's "A case history in scientific method" and those delightful related illustrations "Pigeon in a pelican" and "Baby in a box" (1959) are fascinating and humorous accounts of parts of a research serial. Mills' exploration of The Sociological Imagination (1959), and particularly his conception of intellectual craftsmanship is a major perspective on doing social science. Kenneth Beittel's Alternative Methods for Art Education Research appears at several points in the present essay, and even more in the account of An Evolving Logic of Participant Observation (Smith 1978). Finally George Homan's autobiographical account in Sentiments and Activities and his history of the Society of Fellows (with Bailey 1959) are provocative case studies. Just alluding to these makes me think I should have made a much stronger thrust on the role of models in the origin and evolution of field study problems. But that's one of the beauties of serials; there'll be a next time.

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Observer Role and Field Study Knowledge— An Essay Review of *Usable Knowledge* and *SAFARI I*

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Essay reviews are an uncodified form of professional communication. Our usage is essentially flattering. We propose to report on two recent positions in education and social science, to partially integrate them, and most importantly, to build them into an extension of our own approach. *Usable Knowledge* is a short book by two well-known American social scientists and policy analysts, Charles Lindblom of Yale and David Cohen of Harvard. *SAFARI I: Success and Failure and Recent Innovation* is a collection of papers edited by Barry MacDonald and Rob Walker, educationists from the University of East Anglia, Norwich, England. The Lindblom and Cohen effort poses a research agenda on the interdependence of research, theory and practice in social science. The authors are critical of the conventional wisdom among social scientists who believe professional social inquiry, with its pursuit of authoritativeness, should be the major contributor to social problem solving. The

MacDonald and Walker volume is a series of methodological essays written to explore the nature of educational evaluation, based on experiences accumulated over several projects during the last decade at the Center for Applied Research in Education. The authors, who have been leaders in the case study movement in Britain, have pointed sensitively to the political nature of evaluation, the epistemology of case studies and the ethical problems involved in rethinking this genre of theory and practice. Our analysis and discussions will focus primarily on MacDonald's paper in *SAFARI I*, "Evaluation and the Control of Education" wherein he argues that "evaluation and research are primarily political activities."

As to our own work, the three of us (along with Paul Kleine) are involved in a project we are calling *Innovation and Change in American Education, Kensington Revisited: A 15 Year Follow-Up of an Innovative Elementary School and Its Fac-*

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The three major publications in *SAFARI* are *SAFARI I*, *SAFARI II* (Norris, 1977) and *Changing the*

Curriculum (MacDonald & Walker, 1976). The latter is, in effect, the substantive final report on the follow-up of the four curriculum projects: Humanities Curriculum Project (HCP), Nuffield Secondary Science, Project Technology, and Geography for the Young School Learner (GYSL), studied in the *SAFARI* project. The Norris volume, *SAFARI II*, is a second set of methodological papers which blend the insights from *SAFARI* into UNCAL, *Understanding Computer Assisted Learning*, a subsequent major evaluation effort directed by MacDonald and carried out at CARE.

ulty. In this research we have returned to Kensington, a school we studied 15 years ago (Smith & Keith, 1971). We are pursuing what at one level are relatively simple questions, "What's happened to the school and the original faculty? In what ways are the school and the faculty different now?" At another level, and as we inserted a "Why" question, we found ourselves moving into the heart of very complicated issues in general social science and educational research, theory, and practice and in field methods in particular. Along the way in this enterprise, we have found ourselves stimulated by a number of individuals, conversations, and formal statements. Two of these have been *Usable Knowledge* and *SAFARI*. Typically, as we carry out the practice of qualitative research, we find ourselves looking to colleagues, such as these, for insights, pointers, and alternative perspectives. In a sense we feel like craftsmen and artists, practicing our trade or art form and looking to others in the guild for the multiple kinds of help such peers provide. The artistic and craft metaphor runs through much of our thinking.²

Our intent is to take several of their ideas, and several of ours, compare and contrast them, and build them into a position on "Observer Role and Field Study Knowledge." We hope the synthesis will be interesting enough to provoke each reader into reading the original volumes and to carry out their own reconstrual as it relates to their research and professional activities. Such is our conception of an essay review.

The Attempted Synthesis

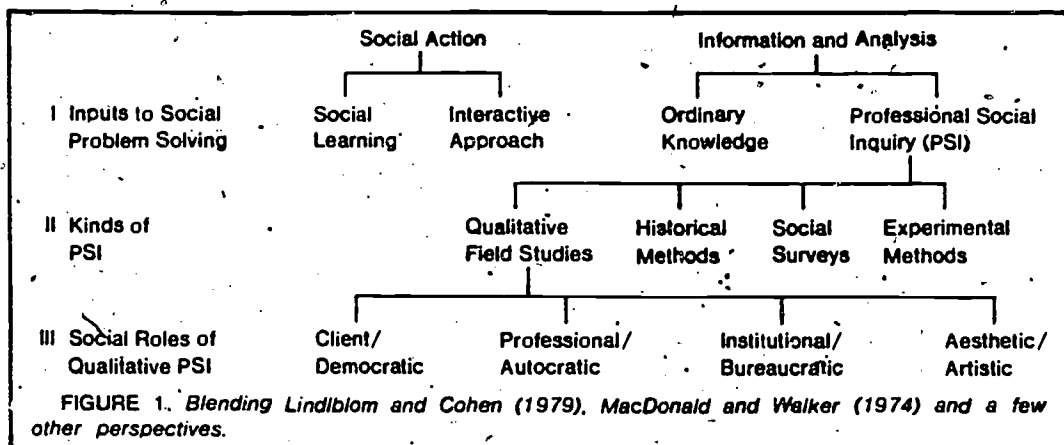
To facilitate the discussion, we have prepared an initial synthesis (Figures 1 and 2). Most of the remainder of the paper is an attempt to talk our way through the Figures. By way of introduction, three aspects seem noteworthy. First, the Figures blend some of Lindblom and Cohen, some of MacDonald and some of us. (In addition,

² Recently, Howard Becker (1974, 1978) has developed a stimulating sociological analysis of the metaphor and Alan Tom (1980) has pursued it in education.

our point of view is the product of many stimulating discussions with colleagues at the GIE and at the recent Cambridge Evaluation Conference III.) Second the conceptual elements are mostly taxonomic, ideal types, or parts and wholes, a mode of thinking that we don't find as comfortable as variables, dimensional analysis, and antecedent and consequence thinking. Third, in Figure 1 the part-whole scheme accents nested processes. Fourth, in Figure 2, we turn the various parts of the taxonomy back upon other parts of the taxonomy, sometimes considered as processes and sometimes as results.

Briefly, then, to turn to Figure 1, several aspects may be noted. Level I is our understanding of one of Lindblom and Cohen's major points: social science knowledge is only one of several inputs to the solution of social problems. The alternative contributions are the ordinary knowledge possessed by most of us growing up in our culture, a variety of interactive approaches such as flipping coins, voting (as in elections for national leaders), or market decisions (as in the flood of Japanese car imports), and social learning perhaps best illustrated by the solution to the energy problem by learning new habits, attitudes and life styles. Just as we are writing this, the St. Louis Public Schools are under a court order to extend the desegregation of the schools by next fall. Among the district's responses is sending a team from the school district, to visit comparable cities—Milwaukee, Indianapolis and Louisville—to observe and talk with school personnel. In the Lindblom and Cohen taxonomy this "input to social problem solving" is a fifth category, "Experience of Professional Peers." It seems a blend of common sense (of a particular group of practitioners), interactive procedures, and truncated qualitative field methods. It is a far cry from "the literature" and "hard data" of the social survey or experimental variety. It echoes Lindblom and Cohen's (1979) introductory comment:

In public policy making, many suppliers and users of social research are dissatisfied, the former because they are not listened to, the latter because they do not hear much they want to listen to. (p. 1)



Perhaps, Lindblom and Cohen's most basic conclusion is that Professional Social Inquiry (PSI) is considerably less important than most of us who are social scientists would hope and like to believe. They seem to imply further, that this is probably a good thing as well. If they are right, those of us who are more action oriented may be troubled by how little influence we have on social problem solving or public policy in action.³

In Level I, Lindblom and Cohen distinguish between two kinds of knowledge: ordinary knowledge developed by common sense procedures such as casual empiricism and reflection, and knowledge generated by PSI. Some social scientists have pushed very strongly on this distinction and argued the importance of the associated technology of control groups, valid measures, random samples, Type I and Type II errors, and so forth. Others, following the lead of Dewey and Kaplan, have argued for the continuities among these two ideal types. We believe, along with Lindblom and Cohen, that part of the current positivist, post-positivist and what Campbell (1979) is now calling the post-post-positivist debate lies here. In brief, major epistemological issues are at stake.

Level II is our overly simple taxonomy of kinds of PSI. Our point in introducing this level of analysis is a simple one: the

³ In our *Federal Policy in Action* (Smith & Dwyer, 1979) we relate and analyze in detail an instance of policy in action and make an argument for its similarities with social problem solving.

diversity in kinds of PSI opens to question any generalization about the undifferentiated category which Lindblom and Cohen call PSI. If we were true believers we might say, "All the negative aspects of PSI detailed by Lindblom and Cohen belong to the practitioner of the other three brands, and not to us who are doing qualitative field studies." (For an extended discussion of the Campbell position vis-a-vis qualitative field methods see Smith [1979].) In a slightly more restrained way, we would argue that the qualitative field study mode often implies several activities. First, there is a rapprochement with ordinary knowledge and common sense inquiry. Stated negatively (at least in some persons' eyes), we are collecting anecdotal records; stated positively, we are tapping the language and perspectives of the participants in the settings, usually educational practitioners. Secondly, the stories we tell, the images we develop and portray enable the reader to engage fully, though vicariously, in the experience itself and to test our interpretations with their own common sense. Thirdly, insofar as the problems and issues are practice based or oriented, as opposed to discipline based or oriented, the nature of the PSI is drastically changed. Perhaps we are only echoing the long-standing applied researchers' lament regarding the utility of basic research. Finally, we believe that the kind of data and ideas generated by qualitative modes, insofar as they accent the concrete and particular, the interactive processes over time, and the contextual and systemic

aspects, are especially useful to policy makers, men and women of action.

In sum, we are noting, only briefly here, the expanding discussion on the relationship between ordinary knowledge and PSI knowledge. Several of the more exciting papers in that debate are Paul Meehl's (1971) "Law and the Fireside Inductions: Some Reflections of a Clinical Psychologist." The fireside induction is a lawyer's label for:

[S]ome mixture of a) personal anecdotal observations, b) armchair speculation, c) introspection, and d) education in the received traditions of Western culture. (p. 76)

He cautions psychologists to be wary of experimental data and theory that purport to refute such generalizations when they are held widely over time, place, and individuals. Donald Campbell (1974), while remaining an experimentalist, takes a slightly different tack and argues for qualitative knowledge undergirding more quantitative experimentally derived knowledge. For an anthropological perspective, we would urge you to consider Clifford Geertz's (1975) paper "Common Sense as a Cultural System." The attributes of common sense which he explores are naturalness, practicalness, thinness, unmethodicalness, accessibility and earliness. Knowledge with those attributes is closely related to action, practice, and use.

After hypothesizing all these good things about qualitative field studies in general, we move to Level III of Figure 1 and present our reinterpretation of MacDonald's scheme of the multiple stances or roles played by practitioners of qualitative PSI. We may be doing him several kinds of injustice. First, his taxonomy has to do with education evaluators, a category he distinguishes from educational researchers. We dissolve that distinction. Second, we have changed his labels, in an attempt to destigmatize the activities and to legitimize each role. Finally, we have added a fourth category to augment the scheme.

If we understand his position, the underlying dimension has to do with power and the political processes enmeshing the research/evaluator. In the client/demo-

cratic stance, the subjects or participants in the study have final control over entry, definition of problems, procedures, ownership, and release of data and interpretations, and so forth. They are the audience of the exercise. The negotiation process between the researcher/evaluator and the multiple individuals and subgroups involved in the program or activity is escalated in importance, focus, and time involvement. In an important sense, MacDonald is advocating political change in liberal, democratic, national states by exposing gaps between the realities of educational settings and the idealism most of us profess in our ideologies. Institutional/bureaucratic researchers are "company men," persons who work for whatever institution (government, university, school system) which hires them. In MacDonald's perspective, she/he helps maintain the status quo. In our perspective, that may be good or bad depending on the institution and its moral and ethical basis and status. We are reminded of the University Bureaus of Institutional Research which in the best of circumstances have made universities more diverse and humane institutions of learning.

The professional/autocratic mode refers to a research and evaluation perspective whose orientation is to the professional research community. At its worst, its inquirers observe and write for each other and ignore the continuing problems in the public schools. At its best, they develop a specialized role for knowledge development and utilization and the training of young teachers, administrators, curriculum developers and other practitioners. Control rests in the hands of research peers. The aesthetic/artistic category, role 4 in Figure 1, is a dream or nightmare of individualism. The metaphor is the artist who is attempting to capture a private vision of reality which might creatively illuminate a part of the educational world. Concepts such as artistic causality, idiosyncratic meaning, intentional symbolization and artistic serial come to mind (Beittel, 1973). Others might see narcissism, elitism, and, as we have indicated, a rampant individualism.

Some Implications of the Synthesis

For the moment, if we assume we have

got MacDonald and Lindblom and Cohen right, and have added a bit of ourselves, then we can turn to Figure 2 and extend the analysis and synthesis. In more colloquial language we are facing the questions: (1) What is the social context of our inquiry? (2) What have we learned from our study? and (3) What do we do with those learnings? In this figure we are speculating, guessing, and hypothesizing about social inquiry. And maybe we are also more involved with possible hopes and intentions than with the realities of what we as researchers and field workers do and what we achieve.

In Figure 2, we have expanded greatly the category of PSI into a taxonomy of results that might accrue from the different modalities of research and the various field worker styles. Just to make one brief point, Lindblom and Cohen would argue that PSI, and especially the experimental mode, seeks to develop authoritative knowledge, the falsification of hypotheses or propositions. In our view (and theirs) that is only one aspect of the possible knowledge results of PSI. We believe the practice of qualitative inquiry has helped expand the conception of possible outcomes to educational research. Along with

FIGURE 2: Hypothesized relationships among social roles and contributions to various inputs to problem solving.

Inputs to Social Problem Solving		Observer Role			
1. Professional Social Inquiry (PSI)		Client/ Democratic	Professional/ Autocratic	Institutional/ Bureaucratic	Artistic/ Aesthetic
1. Portrayals,		High	Variable (low to high)	Variable	High
2. Thick Description Context Multiple Perspectives		High	High	High	High
3. Authoritative Knowledge Concepts Propositions Theories: Miniature to General		Low	High	Variable	Variable
4. Problem Redefinition, Analogies, New Metaphors		Low	Variable	Low	High
2. Social Learning					
1. Immediate	Habits Skills Standard solutions	Immediate	Long Term	Immediate	Long Term
2. Long Term					
3. Vicarious					
3. Interactive					
1. Control of entry procedures, interpretation		Client	Profession and Researcher	Bureaucratic Hierarchy	Re- searcher
2. Beneficiary/Audience		Client/ Underdog	Profession	Bureaucracy	Researcher
3. Researcher Time Involvement (Negotiating Processes vs. Data Collection and Analysis)		Continuing Negotiation Processes	Data Collection and Analysis	Data Collection and Analysis	Idiosyn- cratic
4. Ordinary Knowledge		High	Low	Mid	Variable

a number of our colleagues, we argue that portrayals³ (Stake, 1974), thick description (Eisner, 1977; Geertz, 1973; Ryle, 1971), the generation of grounded theory (Glaser, & Strauss, 1967; Glaser 1978), and problem redefinition, analogies, and new metaphors (Rein & Schön, 1977) are important outcomes of PSI.

It is Figure 2 that we would like to explore a bit more. It is here that the MacDonald position vividly and productively strikes sparks with the Lindblom and Cohen positions. It is our contention that the client/democratic role represents a fundamental blend of PSI and the interactive inputs to social problem solving and policy action. Essentially, the stance or role changes the interrelations among the various actors in the research, theory, practice enterprise. The control of entry, procedures to be used, ownership, feedback and release of data, and final say on interpretation lies with the individuals most directly involved and most likely to be hurt by communication of information. These individuals tend also to be those with least power in the system. MacDonald's stance typically empowers them.⁴

In our recent work we have tried to implement various approximations of this stance.⁵ For instance, in our *Federal Policy in Action* study (Smith & Dwyer, 1979) we engaged in formal, written consent procedures; we wrote a first draft of the document which was circulated to all the participants we interviewed; we then rewrote the final draft, and we also, by contract, agreed to append addenda by three key figures in the project if they desired to write them. The trade-offs were large. The report expanded 20 percent, from 429 to 509 pages. We worked another 6 months for which we had no budgeted resources. We were over a year late in meeting the legal aspects of the contract. Nonetheless,

⁴ Even here a major discussion exists as the Becker (1970) and Gouldner (1968) debate on underdogs, overdogs, and middledogs and the question, "Whose side are you on?" indicate.

⁵ Actually, the first piece of field work carried out by any of us was the Smith and Geoffrey (1988) collaboration, which began in a University class of Smith's, was stimulated by Geoffrey's concern, carried out in Geoffrey's class, and ended in a joint publication.

we feel we have a better report. The participants who have read the final draft feel that we responded to the prior comments and have a fuller, more complex and subtle view of the multiple perspectives involved in the project. In our view, we have a thicker description. We have lingering questions as to whether that time would have been spent better teaching at the University, working with children and parents, or consulting with other researchers and practitioners, all of which were available options. As Parlett (1977) in *SAFARI II* says more pithily, "Some very hard thinking is thus called for: something has to give, a pint pot can only take a pint." (p. 48).

Briefly, we would make the case for the alternative styles, especially as they contrast with the client/democratic stance. The professional/autocratic role, one which we have been partial to, takes a much longer time perspective and a more distal view of settings and audiences. In our university setting, we focus on portrayals, thick descriptions and the generation of grounded theory. We have students to teach, both graduates and undergraduates. If they can see Geoffrey's classroom, the Kensington Elementary School, or the multiple realities enclosed in our *Federal Policy in Action* study, and if it enhances their perspective on education, we will be pleased. Similarly, if it can occasionally influence other researchers, theorists, and occasional practitioners, we will be pleased. Some parts of the world, we believe, are changed by books, conferences and conventions, and by the teaching-learning process.

Those of you who have been a part of the 10-year debate over formative and summative evaluation in curriculum development can easily see the issues of observer stance. On some occasions, while doing more formative evaluation, one's focus is more institutional/bureaucratic in moving a product from its initial, early "messing around" stages to pilot and extended pilot trials. On other occasions the focus is on direct help to teachers and pupils engaging in the teaching and learning process with the new materials. The stance promotes active, immediate help—usable knowledge.

Finally, we offer a word in support of a

more freelancing, autonomous style, the artistic/aesthetic role. The label comes from our image, perhaps idealized, of the artist pursuing and transforming his or her own private vision of the world into a public product. Perhaps we are frustrated painters, poets, or composers, but we find the image provocative and appealing. The one aspect that we would point to is another blend of method and practice. When we have taught experienced educators—teachers and administrators—to study their own classrooms, schools, or organizations using participant-observer techniques, a large number have found their creativity and intelligence engaged dramatically. We can see it in the sparkle in their eyes, in the excitement in their voices, and in the awkwardness in not wanting to seem maudlin or sentimental in reporting that significant emotional events are occurring in their lives and in their interactions with colleagues and pupils. We believe it has something to do with what Schaefer (1967) once called *The School as a Center of Inquiry*. We believe it is one illustration of a significant kind of social learning as an input into social problem solving as Lindblom and Cohen defined those concepts. And we believe it is very important.

Conclusions and Implications

In this essay review we have considered issues in the usefulness of educational and social science theory and research. And we have considered how observer roles in qualitative field studies yield multiple kinds of usable knowledge to a variety of audiences. By focusing on qualitative field methods and by synthesizing the efforts of Lindblom and Cohen, MacDonald, and ourselves we believe we have extended the positions of all three. In concluding an exercise such as this we must question the practicality of the perspective we have proposed. In our own experience, the observer role has been an important consideration in the procedures and methods we employ in the field. Our understanding of the multiple qualitative stances also influences the way we train and socialize students in educational research methods. When we are asked to review proposals or

referee articles, we find our perspective useful in making recommendations for or against funding or publication.

Finally, several implications warrant a brief statement. First, the problems of research, theory, practice and their interdependence may be old chestnuts but they have a currency and fascination worthy of their importance. The educational world is in ferment and flux. We find MacDonald and Lindblom and Cohen an exciting part of that world. Second, we believe that much of the discussion of field methods, social problem solving, and policy process implications assumes a too simplistic view of the relationships among organizations, people, ideas, and inquiry purposes. In our experience in multiple kinds of field studies we have found that our professional relationships with any one individual can vary among being collaborator, project subordinate, project superior, research subject, instructor and student, reviewer and critic, sponsor, monitor, decision maker of several sorts. This tangle has increased over the years. We have found that ideas cumulate across times, settings, problems, individuals and organizations. In most all of the projects in which we have been involved, multiple purposes within and among abound. Sometimes the results have immediate consequences in action; more usually they have longer term practical consequences in our teaching. We try to cope with this tangle with a "committed relativism."

Third, in our experience, most relevant others, teachers, administrators, FEDs, university colleagues and administrators, seem willing to accept and/or negotiate different kinds of roles depending on the problem, the situation and the contending interests. Seldom have we found the stances given, that is, not open to discussion. Fourth, our own practical social problem solving and policy activity seems to occur in several social settings: (1) In classes, seminars, and tutorials enrolling practitioners and policy makers; teachers, school administrators, evaluators, project directors, et al.; (2) In personal relations, "kitchen cabinets," and advisory committees with principals, superintendents, deans, chairmen, center directors, project directors, federal officials; (3) In collegial groups in AERA, CAE, and informal net-

works; (4) In formal lectures, symposia, colloquia and meetings.

"Successful" social problem solving and policy activity is not easy to define for the activity is not a zero sum game but an individual and organization thinking, acting, and rethinking of strategy and tactics in a social context. We are arguing that social and educational action in a democratic society is more complex than most models espouse. We believe that qualitative field methods play an important part in coming to grips with that complexity and, moreover, the diversity of observer roles may increase the yield of practical, useful knowledge to a variety of consumers of PSI, not least of which are those concerned with improving schools.

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3.4 Archival Case Records...

Archival Case Records:
Issues and Illustrations^{1,2}

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¹These materials have been prepared for the S.S.R.C.: Exemplary Case Records Project Conference, York, England, September 5-8, 1980.

²Supported in part by NIE Grant #G78-0074, Kensington Revisited. The perspective/presented does not necessarily represent the policy or point of view of NIE or the Milford Public Schools, and no inferences should be made to that effect.

1. Introduction

The very preparation for the conference on Case Records requires a series of decisions which highlight many of the issues that the conference will be treating as problematic. The first of these issues might be "Which case study project among the several with which we've been involved, should we pick?" For a number of reasons I chose our current project "Kensington Revisited." We might want to explore the reasons at the conference. A second question, "Which records to include in the package of material?" is even more important and problematic. I finally decided to try and capture the entire process of the case study from the beginnings of the idea to preliminary drafts of the final report. I don't know whether that was a wise decision, but it seems that that too, is something we need to talk about. A third decision has to do with "sampling" the totality of possible materials. So far in the Kensington Project we have several file drawers of data. We couldn't bring all of that so we sampled. A fourth issue involved "tidying up" the materials. We deliberately left some items just the way they are - handwritten, partially reworked, and so forth. Some of it may be unreadable, illegible, isolated and unconnected. And that's an interesting and important archival problem. Fifth, and perhaps our most difficult problem, we've tried to anonymize all the documents we've included. That's taken considerable time. It involves issues in the rights of human subjects, informed consent, and potential harm to the people in our settings. That issue, above all others needs discussion. Finally, these materials were collected originally "for research purposes", which is a "funny" set of underlying intentions, guiding the selection, storage and now reproduction of the items.

2. The Illustrative Items

In one sense, the most important item for the archives is our book Anatomy of Educational Innovation (Wiley, 1971). This is our ethnographic case study description and analysis of the first year in the life of an innovative elementary school. The book is now out of print which poses some additional problems for archival records.

In this set of materials for the case record we have included the following ten items. They are introduced here with a brief explanatory sentence.

1. Kensington Reconsidered

A paper for an AERA Symposium which helped generate the idea for the study.

2. Preliminary Proposal "Kensington Revisited"

A response to an NIE Request for Proposals which scored well enough to encourage a full proposal and eventually provided support for the project.

3. Observer Role and Field Study Knowledge

An AERA paper growing out of the research which was part of a continuing attempt to formulate the methodology used in our work. Our largest effort in this regard was "An evolving logic..." (See references Smith 1979)

4. Progress Report 11/26/79

Continuing communication with NIE is required in our contract. The document illustrates a number of important aspects, e.g. the flow of outside stimulation into the project, the time press, etc,

5. Historical Document - Excerpt from 1931 High School Annual

In our work we collect as many local documents as we can find. This is part of the historical context.

6. Interviews

In our studies we have interviewed dozens of people connected with the school. We have excerpted a couple of pages from an interview.

7. Summary Observations and Interpretations

We make great use of tape recorders, especially as we drive to and from the site. We dictate observations in a free associational style and make whatever interpretive comments which come to mind.

8. Field Notes

In the setting - classroom, school, central office we scribble, at the time, notes of the who, what, when, where variety. Our final reports are full of these. Some of the best classroom examples are in The Complexities of an Urban Classroom. We've included a page or two from a primary grade group at Kensington.

9. The Beginnings of a Theoretical Model

One of the most exciting parts of case study inquiry comes when the particulars of the case seem to pattern themselves and seem to generalize and speak to larger issues.

10. Preliminary Draft of Final Report - An Excerpt

Our project reports literally evolve in what seems a quite haphazard, grounded, inductive style. They go through several editions. We enclosed an early typing of part of an historical piece we've been working on this Spring. It's rough.

3. Conclusion

With these brief introductory comments and the assorted items, I hope to have given you a view of the inside of one of our projects and how we go about doing what we do in case studies, field work, or ethnography. These fragments should enable us to talk quite specifically and realistically at whatever length you desire about one particular project. In addition, it should provide a stepstone to develop comparisons and contrasts with any project that anyone has done, is doing, or contemplating. With a little luck that should coax out the best of our creativity and engage us in some exciting discussion and conversation. And that's my understanding of why we're here.

4. ETHNOGRAPHY AND RELATED INQUIRY

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Historically, the term "ethnography" has referred to the set of field-research methods used by anthropologists in the scientific study of primitive, nonliterate cultures (Conklin, 1968). As such it might be contrasted with the laboratory experiment, which has been the prototypical psychological method, or the social survey, which has been the defining methodology of much of sociology. In professional education, as in the disciplines of anthropology and sociology, adaptations, extensions, and new syntheses have arisen in response to an ever changing agenda of problems, cross-disciplinary efforts, and realignments of research communities. At the present moment one of the most fundamental observations about the concept of ethnography, as used in education, is that it is evolving rapidly. This seems related to an explosion of activity from several research communities that are only partially in communication with each other. Delamont and Atkinson (1980), Hammersley (1980), and Smith (1979) summarize some of this diversity. Accompanying this rapid increase in research is a proliferation of conceptual distinctions, with accompanying labels, that contain ethnography as the root concept. Table 1 presents a list of terms and their recent exponents or commentators.

Although these variants of ethnography present their own diversity, it is important to realize that ethnography itself is only one label for several social science methodologies, used by different disciplines and in different research communities, that have a number of fundamental similarities. Quasi synonyms of "ethnography" include "case study," "field study," "naturalistic methods," "participant observation," "responsive evaluation," and "qualitative methods." Wax (1971) sees a continuity both historically and conceptually among ethnography, participant observation, and field study. She titled her own book *Doing Fieldwork* (1971). Within educational ethnography, Wolcott (1975, 1980) is the most vigorous exponent for careful delineation of labels and restriction of the term "ethnography" to a type of fieldwork whose problems, methods, and results are empowered by and illuminated through anthropological concepts. In contrast, one of the earliest and most influential educational ethnographers, Spindler (1981), is arguing now for a more differentiated conception—anthroethnography, psychoethnography, and socioethnography—reflecting the multiple disciplines and theoretical positions now allied with ethnography. Any inquirer surveying the literature in the general domain is advised to check indexes for all the variants and quasi-synonyms.

Exemplars of Methodology. When a field is in such zesty disarray it seems useful to present several exemplars, from different times and places, that have implicitly served as models for several generations of researchers. These exemplars are drawn from the ranks of the more classical anthropological practitioners—Malinowski, Whyte, and Geertz—rather than from current educational research-

ers. Each has done a major ethnographic study, and each has written articulately and influentially about his use of methodology.

Any students, whether neophytes or experienced scholars, pursuing the concept of ethnography cannot go far wrong by beginning with Malinowski's classic introduction to *The Argonauts of the Western Pacific* (1922). He comments: "One of the first conditions of acceptable Ethnographic work certainly is that it should deal with the totality of all social, cultural, and psychological aspects of the community, for they are so interwoven that not one can be understood without taking into consideration all the others" (p. xvi). So Malinowski states his holistic and functionalist position. He argues that ethnography depends on real scientific aims, on living right among the natives, and on using a variety of special methods of collecting, manipulating, and fixing evidence. In elaborating these ideas, Malinowski provides distinctions, images, and metaphors that have rallied anthropologists for six decades, and educational theorists more recently. These include (1) foreshadowed problems versus preconceived solutions; (2) methods of statistical documentation: by concrete evidence, charts, and synoptic tables; (3) collecting concrete data on real cases, drawing inferences for oneself, and pursuing the data relentlessly and completely; (4) blending observation, constructive work (analysis and writing), and empirical checking; (5) focusing on the imponderables of everyday life; and (6) penetrating the mental attitude of the subjects. The ultimate goal, if it needs stating in a simple final form, is "to grasp the native's point of view, his relation to life, to realize *his* vision of *his* world" (p. 25). Although the idealism of this has been tarnished a bit by Malinowski's diary (Malinowski, 1967; Geertz, 1967; Wax, 1972), the ethnographic model remains. Educational researchers approaching communities, schools, and classrooms remain well served by his ideas and ideals.

Two decades later William Foote Whyte, working in the unusual context of the Society of Fellows at Harvard University and under the tutelage of more behavioristically oriented anthropologists (Arensberg, Chapple, and Kimball), began *Street Corner Society: The Social Structure of an Italian Slum*, the first of a series of his investigations into contemporary society and its institutions. Later he published a major methodological statement, "On the Evolution of *Street Corner Society*" (1955). Much of that essay was an attempt to give one account of the actuality of field research as he experienced it, which, he hoped, when compared with other such accounts, would lead to a theory of research methodology. As he said: "I am convinced that the actual evolution of research ideas does not take place in accord with the formal statements we read on research methods. The ideas grow up in part out of our immersion in the data and out of the whole process of living" (p. 280). A careful reading of Whyte's account suggests several major ideas about ethnography as he practiced it. First,

TABLE 1. Recent variants of "ethnography" among educational researchers

Variant	Researcher(s)
Anthroethnography	Spindler, 1981
Anthropological educational ethnography	Delamont and Atkinson, 1980
Anthropological ethnography of schooling	Spindler, 1981
Anthropopedagogy	Morin, 1980
Blitzkrieg ethnography	Rist, 1980
Classical ethnography	Mehan, 1980
Classroom ethnography	Hammersley, 1980
Constitutive ethnography	Mehan, 1978
Contract ethnography	Wolcott, 1975
Cooperative ethnography	Hymes, 1980
Educational ethnography	Spindler, 1981
Educational ethnology	Hymes, 1980
Ethnographic approach	Fitzsimmons, 1975
Ethnographic case studies	Herriott, 1977
Ethnographic methods	Lutz, 1980
Ethnographic monitoring	Hymes, 1980
Ethnographies of classroom life	Hamilton, 1981
Ethnography and policy making	Mulhauser, 1975
Ethnography of schooling	Wolcott, 1975
Ethnopedagogy	Burger, 1971
Evaluation ethnography	Rist, 1980
Focused ethnography	Erickson, 1977
Macroethnography	Lutz, 1980
Microethnography of the classroom	Smith, 1967
Neoethnography	Bullivant, 1978a
New ethnography	Erickson, 1973
Psychoethnography	Spindler, 1981
Socioethnography	Spindler, 1981
Sociological educational ethnography	Delamont and Atkinson, 1980

the problem of his research evolved from an undifferentiated "vague idea" of studying a slum community into a set of ideas linking intensive analyses of small social groups to broader issues in the larger community. The micro-macro problem became the issue. Second, the evolution was facilitated by such factors as the comments of his colleague John Howard on the theme of leadership (p. vii); the kinds of relationships he had developed, especially with "Doc," a leader of an informal group in Cornerville (pp. 291-292); the participatory style of his involvement with his subjects (pp. 302-303); and the preponderance of the kinds of data he was accumulating (p. 308). Finally, Whyte presents an image of one kind of criterion for ethnographic research: "It was Henderson [senior fellow and professor of biochemistry] who was easily the most imposing figure for the junior fellows. He seemed particularly to enjoy baiting the young social scientists. He took me on at my first Monday night dinner and undertook to show me that all my ideas about society were based on soft-headed sentimentality. While I often resented Henderson's sharp criticism, I was all the more determined to *make my field research stand up against anything he could say*" (p. 288, italics added). If we generalize this comment, good ethnographic research should interrelate issues, data, ideas, and conclusions so as to withstand the most critical assaults of an analytical father figure.

If Malinowski's position can be categorized as "functionalist," and Whyte's as "behavioral" or "social interactionist," Geertz (1973) falls into another theoretical tradition: that of an interpretive perspective. In his sequence of ideas, anthropology's mode of inquiring is ethnography; ethnography is "thick description"; thick description is the untangling of knotted nets or webs of meaning; the webs of meaning are culture; and culture is the subject matter of anthropology. In his own words: "From one point of view, that of the text book, doing ethnography is establishing rapport, selecting informants, transcribing texts, taking genealogies, mapping fields, keeping a diary, and so on. But it is not these things, techniques and received procedures, that define the enterprise. What defines it is the kind of intellectual effort it is: an elaborate venture in, to borrow a notion from Gilbert Ryle, 'thick description'" (p. 6). In its simplest form, the argument of Geertz uses an illustration of Ryle's notion that understanding culture is much like distinguishing the involuntary twitching of an eyelid from winking, from parodying a wink, and from practicing a wink or a parody of a wink. "Contracting your eyelids on purpose when there exists a public code in which so doing counts as a conspiratorial signal is winking. That's all there is to it: a speck of behavior, a fleck of culture, and—voilà—a gesture" (1973, p. 6).

Thick description is the ascertaining of these multiple levels and kinds of meaning. If that seems too simple, follow Geertz in his brief account of Cohen and the Moroccan sheep raid (1973), the deep play of the Balinese cock fight (1973), his analysis of common sense (1975a), or the nature of art (1975b). Quattrocento painters who write mathematical treatises on "gauging" and Islamic poets who engage

in "agonistic interpersonal communication" exemplify and enlarge, according to Geertz, the complex webs of cultural meaning. The purpose of all this activity, in the search for meanings in the historical or contemporaneous world in which one's subjects live, is to be able to "converse with them." As one penetrates more and more deeply into a culture, the language one develops expresses finer and finer shades of meaning. The theoretical abstractions one creates then are reapplied to the reality one is investigating to develop further insightful thick description. The play of this kind of science into the more general problems of mankind and into a special view of the human condition is seen in Geertz's summary comment: "The essential vocation of interpretive anthropology is not to answer our deepest questions, but to make available to us answers that others, guarding other sheep in the valleys, have given, and thus to include them in the consultable record of what man has said" (1973, p. 30). In Kluckhohn's distinguished phrasing, "Anthropology holds up a great mirror to man and lets him look at himself in his infinite variety" (1949, p. 11).

In sum, ethnography as presented and rationalized by Malinowski, Whyte, and Geertz, in their substantive contributions and in their autobiographical and methodological accounts, accents several major perspectives and orientations: (1) living for an extended time in the community or with the group being studied, which permits the researcher as a person in his or her direct participation, observation, and note taking to become the major source of data; (2) a concern for the small, mundane, day-to-day events as well as the esoteric or "important" events in the lives of individuals living, working, and playing together; (3) a particularistic focus on these individuals' perspectives, meanings, and interpretations of their world; (4) an attempt to build a synthetic and contextualized if not holistic view of life in the community, organization, or group; (5) a tendency to intentionally view the interpretive or conceptual structures in the research as evolving, being discovered, or being construed throughout the course of the research from the first statement of the problem to the final report; (6) reporting through a conscious and creative blending of storytelling or narrating with more abstract conceptualizing or theorizing. These three major investigators, in their reports and methodological accounts, implicitly and explicitly differ in their theoretical perspective and metatheoretical assumptions—in what Sanday (1979) has called "the style of ethnographic paradigm(s)." Beyond the holistic, semiotic, or behavioral styles, differences exist in breadth (miniature versus middle range versus grand theory), in abstraction (substantive versus formal), in form (coherent versus eclectic), and in purpose (practical versus basic). Such distinctions go beyond ethnography, and perhaps beyond social science, into the foundation of Western thought (Pepper, 1942; Toulmin, 1972).

Exemplars of Educational Ethnography. In the genre and style of Malinowski, Whyte, and Geertz, and illustrating the six generalizations made about their work, educa-

tional ethnographers, from several parts of the world, from several substantive disciplines, and from varied theoretical perspectives have opened almost every facet of education to inquiry. Studies abound of communities (mainstream and minority), of schools (elementary, secondary, tertiary, and professional), of school positions (students, teachers, principals), of programs and curricula (formal, informal, extra, hidden), and of educational processes (curriculum development, evaluation, innovation, socialization, and teaching-learning) (Smith, 1979). For the ethnographer, each variation represents a solution to a complex, interrelated set of issues regarding problem, setting, procedure, focus, theoretical perspective, and research purpose. Table 2 illustrates this diversity and indicates how educational settings have been approached ethnographically. The dozen studies shown in the table provide a set of models for the neophyte and for the experienced researcher who desire a view of educational ethnography in action.

New Directions. Anyone who engages in predictions about the directions of creative scientific effort strains the credulity of the critical reader. Be that as it may, several kinds of activity seem to be in the offing. First, the flow of other social scientists and educational researchers into the provinces once claimed more exclusively by anthropologists will continue. The University of Chicago sociologists doing symbolic interactionist research and the Columbia University functionalists have had a long claim to descriptive-interpretive methods in communities, in organizations, and in life histories. Delamont and Atkinson (1980) vividly describe both sociologically oriented and anthropologically oriented ethnographic work in education in Britain; the former, they say, is "more lively and active." But the group that has overrun the territory recently are the educationists, especially the educational evaluators and educational psychologists, who have found that their tools (tests and statistical procedures), their conceptual structures (trait and behavioral theory), and their settings (laboratories) cannot deal adequately with the interactional, political, contextual, and cultural problems of schools and classrooms. As these workers have abandoned, altered, and adapted their perspectives, both concrete and abstract, they have left the educational research community in disarray over problems, methods, and standards. As they have adopted the "case study approach," they have accented the kind of synthetic and holistic activity, careful descriptive-narrative accounts, and participant perspectives represented by the several major anthropological positions (Hamilton et al., 1977; Smith, 1979).

A second prediction is that efforts to move analytically with constitutive ethnography, focused ethnography, or microethnography on selected issues in schooling and education will continue (Erickson, 1977; McDermott, 1978; Mehan, 1980; Stubbs & Delamont, 1976). In part this will involve increasing use of technology: videotapes of interaction, audio recording of interviews, and computerization of data analysis. Although theoretical accents will vary among neofunctionalism, neobehaviorism, neo-symbolic interactionism, and neo-Marxism, the general movement will collide—productively, it is hoped—with earlier and technically similar traditions of time sampling in child development, systematic observation in social psychology, and quantitative classroom analysis in education (Weick, 1968; Dunkin & Biddle, 1974).

Third, although focal groups, settings, and issues will always be highlighted by ethnographers, educational researchers, with ethnography as a main component, will be arguing for two kinds of context: the larger system or culture in which the educational event is embedded and the historical stream of which the contemporary event is a part. Microethnographers, conservative or radical, will debate among themselves as well as with exponents of macroanalysis; synthesizers will move toward codification (Smith, Prunty, & Dwyer, 1981; Hammersley, 1980; Hamilton, 1976; Eickelman, 1978; Berlak & Berlak, 1981).

Fourth, the perspectives of multiple actors in the educational drama will be presented more frequently than in the past. Pluralism and politics are here to stay. Moreover, the extension of the multiple perspectives to include the researcher, emic versus etic orientations, will continue to occur and continue to be debated. Perhaps it will resolve itself into concepts such as "experience-near" and "experience-distant" raised by Geertz and defined with devastating illustrations: "How, in each case, should they [the concepts] be displayed so as to produce an interpretation of the way a people live which is neither imprisoned within their mental horizons; an ethnography of witchcraft as written by a witch, nor systematically deaf to the distinctive tonalities of their existence, an ethnography of witchcraft as written by a geometer?" (1975b, p. 48). In professional education, which is intrinsically a normative domain, according to some philosophers (Peters, 1965; Scheffler 1971), educational ethnographers not only must phrase the experience-near perspectives of teachers, pupils, administrators, and parents and the experience-distant concepts of researcher-theorists, but must also phrase the more Jovian "experience-good" conceptions—not so light a task.

Finally, in keeping with the differentiated thinking of Spindler (1981), there will surely be an "educational ethnography," whose basic and derived concepts, ideas, and interpretations belong to education rather than to one or another of the social sciences.

In conclusion, educational ethnography in the early 1980s encompasses a domain of great vitality, controversy, and multiple referents. Even among the most highly regarded anthropologists, such as Malinowski, Whyte, and Geertz, the conception of anthropological theory and method, and the interdependence of theory and method, have been evolving as one would expect in any scientific domain. As indicated in Tables 1 and 2, multiple voices suggest that a diverse range of drummers will continue to call to and for ethnographic marchers. Perhaps the strongest benefit will be long-term, in the development of mirrors enabling the multiple participants in the schooling enterprise to see more clearly, think more creatively, converse more empathically, and decide more wisely about their educational lives.

Louis M. Smith

See also Anthropology; Culture and Education Policy; Sociology of Education; Systematic Observation.

TABLE 2. Exemplars of educational ethnography.

	<u>Community</u>		<u>School level</u>			<u>School position</u>			<u>Curriculum</u>			<u>Educational process</u>				
	Mainstream	Minority	Elementary	Secondary	Tertiary	Professional	Student	Teacher	Administration	Formal	Informal	Hidden extra	Curriculum development	Evaluation	Innovation	Socialization Teaching-learning
Becker et al., <i>Boys in White</i> (1961)	X					X	X			X	X	X				X
Bullivant, <i>Tradition</i> (1978b)		X		X			X	X		X						X X
Cusick, <i>High School</i> (1973)	X			X			X				X					X
Hamilton, <i>Structure</i> (1976)	X		X				X	X		X	X			X	X	
Henry, "Attitude" (1957); "Spontaneity" (1959)	X		X				X	X			X					X
Mehan, <i>Lessons</i> (1980)		X	X				X			X						X
Peshkin, <i>Growing up American</i> (1978)	X			X			X	X		X	X	X				X
Rist, <i>Urban School</i> (1973)		X	X				X			X						X
Smith and Geoffrey, <i>Complexities</i> (1968)		X	X				X			X						X X
Wax et al., <i>Indian Community</i> (1964)		X	X				X	X			X			X		
Willis, <i>Learning to Labor</i> (1978)		X		X			X				X					X
Wolcott, <i>Principal</i> (1973)	X		X					X			X					X

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4.2 Ethnographic and Historical Method...

Ethnographic and Historical Method in the Study of Schooling

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1. An Introduction to the Issues

Most essays have an interesting origin, a beginning, which helps give meaning to the substance one wishes to present. Among several, one brief story comes to mind in regard to these thoughts on the relationship between historical and ethnographic research in the study of schooling; it comments on the Department of Education at Washington University where I work. Although most of us are quite individualistic we do talk, banter, and play a variety of friendly one-ups-manship type games. Several years ago I had been trying to understand the concept of "explanation" and began reading people like Scriven (1959), Dray (1957), and Gardiner (1959) on the nature of explanation in history. In the course of this I began to try to figure out how historians do their work. With my colleagues Arthur Wirth, Raymond Callahan, and William Connor in mind, I came to the conclusion that historical method was just, nothing more than, "Participant observation with data fragments", a kind of less adequate ethnography. I don't recall their specific reactions, beyond benevolent, tolerant smiles, and I'm not sure that they believe I won that round. Cloaked in the jest, however, were two significant ideas. Essential similarities existed in the two approaches, that is, one is a form or instance of the other. Second, a major difference seemed to appear in the quality of the data that existed, that is, fragments. Malinowski's (1922) active ethnographic huntsman image seemed bounded. In a sense, my hope in this essay is to explore the hunches caught in that bit of attempted humor.

Such humor and by play suggests also that a more fundamental issue is lurking about. I knew one day I wanted to do a real historical case study. At first, I didn't realize the extent of the opportunity which would develop around our project, Kensington Revisited: A Fifteen Year Follow-up of an Innovative School and its Faculty. In a sense, a fifteen year follow-up is a kind of recent history. We had studied the innovative Kensington Elementary School--open space, team teaching, individualized curriculum and instruction, democratic administration, pupil control of their own learning in 1964-65 and had written a participant observer monograph, Anatomy of Educational Innovation (Smith and Keith, 1971) on the first year in the life of the school. Now we were supposed to return for an ethnography of the school today (1979-80) and a view of the further careers of the original faculty, a group of true believers, none of whom was still at the school.

In looking for some of the context of change it seemed reasonable to spend some time in the Central Office of the Milford School District, of which Kensington is one elementary school, to read documents such as the Milford Public School Bulletin, and to attend some of the School Board meetings. One day, while reading the file of Bulletins that the District sent to patrons, I happened upon some elements of conflict between the then Superintendent, Mr. McBride, and the Board. In talking with the current Superintendent, Mr. George, he indicated that the Board had tried to fire Superintendent McBride. We chatted a bit about the event, he wandered off, only to return a little later with a large black book, which turned out to be the bound Minutes of the Milford District Board of Education, the legal record of school proceedings. I was amazed at the bill of particulars against Superintendent McBride, the

traces of significant human action and interaction. To my questions: Are there more of these?, How far back do they go?, and, Can I look at them?, he said they had a closet full, they go back at least to the 1920's, and that I was free to look at them. For the last couple of years I've been reading them avidly. While, we are still not "historians" in a formal or professional sense, we have been busy developing a chronicle of the Milford District from 1915 to 1980, a sixty-five year period. The activity has changed the definition of our problem, enlarged our final report to six book length volumes, and demanded continued renegotiations with school district officials and with our project officer at National Institute of Education (NIE) which has funded the effort. Most of my concrete illustrations and substantive remarks will come from our experience in this project.

2. Anomalies, Initial Observations and Reflections on Doing Ethnography and History

An interpretive aside is a simple, practical ethnographic technique of writing in the field note records a short comment to oneself about some hunch, bright idea, or insight that occurs along the way in field work (Smith and Geoffrey, 1968). In the course of doing our history of Milford, a series of methodological observations and reflections arose and were noted in the field notes and summary observations. I raise and elaborate a half dozen of them here. Mostly, the focus is on aspects of the data and the kind of thinking that seems to be occurring as one does a blend of history and ethnography.

2.1 The World Begins in 1952-Phenomenon

One of the more interesting items we encountered is "the world begins in 1952" phenomenon. Among the various sources of data on the history of the district the Milford Public School Bulletin became one of

the most important. Volume 1, #1 appeared in August of 1952. That issue of the Bulletin became the fulcrum around which the history of the district seemed to unfold. Each news item gave an indication of what had gone before and what was just beginning. It became a rock upon which inferences ran both backward and forward. For example, what was to become a deluge of pupils in the 1950's, changing the district from a semi-rural, semi-small town district to a large suburban district, was just beginning. The Milford High School listed only 13 faculty members with "one or more years of service"; nine new faculty arrived in the Fall of 1952. The Milford Village Elementary School listed 12 faculty and six new members. The Marquette School of 14 faculty was increased by four. The Attucks School remained with a single teacher for all eight grades. In all, staff size increased by 50% in that year. A two class addition was being built at Marquette. Six classes were being rented from the Adams Street Community Church's Education Building, and the new Grant Elementary School was under construction. Overcrowding in the elementary schools was a real problem.

The substantive points are both interesting and important but my purpose here is to indicate that we found a rock to anchor our beginning story and analysis. The data produced an initial "set of facts" and a beginning image of the nature and structure of the district. This record occasioned the raising of hypotheses about earlier years and the recency of the change from rural to suburban. It began to suggest linkages with the Kensington School and Milford District which we had encountered in our intensive observational study in 1964-65, Anatomy of Educational Innovation. To find a source which one could count on, at

least initially, seemed very helpful. Because they started publishing the Bulletin in 1952, it seemed as though our world began in 1952.

The Bulletins, as data, had several other attributes which helped shape the label "The World Begins..." we gave to the phenomenon. First, the Bulletins were a large mass of data. Some years a half dozen were issued; they varied from four to eight pages in length. They have continued until the present. Second, by their very nature they were organized chronologically. They provided a kind of chronicle of the District for the latter day reader. Third, they had a holistic quality--"all" of the District news of interest to the patrons, at least as perceived by the administration. Fourth, the audience of patrons are the citizens who vote yearly for Board members, who legally direct the district, and the citizens whose support is needed for tax levies and bond issues--local democracy with a vengeance. As we will raise shortly, the Bulletin is not the only perspective possible but is an interesting and important one for an ethnographer interested in documents as well as observations and interviews.

2.2 Discovering The Board Minutes: Cognitive and Motivational Consequences

The initial interpretive aside leading to the previous section, "The World Begins in 1952" was written before we got into the Board Minutes. These became an even more fundamental rock--earlier in time and with a peculiarly important decision making format. It reminded us of the old story of the earth resting on a rock, the rock resting on an elephant, and the elephant on a turtle, and after that "it's turtles all the way down." The Board Minutes seemed to be "turtles" all the way down. But even here, we had not gotten into the community newspapers

and we thought we might find them to be "the real bedrock of turtles" to continue to mix our metaphors. Actually we used those records more selectively:

The point I am trying to make is the nature of the cognitive processes involved in this phase of this kind of research--at least as we are doing it. The finding of these sets of documents and the careful reading of them started several processes. First, as I indicated earlier, it set a boundary, "1952 and onward." Second, it indicated that those boundaries are always provisional and tentative as new sets of records appear. Third, by their amount, detail, and particular form--news items for the patrons in the one instance and legal records in the second, they gave an aura and confidence of "reality". Fourth, as I started to make notes I found myself chronicling specific events on a time line. Fifth, common sense strands--when buildings were built and the coming and going of Superintendents soon appeared. Sixth, some specific items related to contemporary events became themes, e.g. a Black School was built in the mid 1920's and reference was made to it in one of the first volumes of Board Minutes. This linked with the contemporary racial changes at Kensington and with what Myrdal (1944) long ago called The American Dilemma. Seventh, a developing gestalt or image of what the totality or whole is like began to form. Something called the Milford School District existed in the records, in the legal settings of county and state, and in the minds of individuals. We found ourselves in the midst of the Griffiths (1979) and Greenfield (1978) debate. Eighth, a continuous inference process about "unknown areas" based on implications from known items occurred. For instance, what was the relationship between faculty and staff turnover in Mrs. Briggs

early tenure as Superintendent in 1928-30 and in her being fired after two years? What relation did the Board's stated reasons have to her personality? What was the significance of pupils being called in by the Board to discuss the problems of the school? Finally, what alterations occurred in each of these processes as new information, and particularly new large bodies or sets of information arose, for example finding the District Bulletin, then the 65 years of Board Minutes and then the extended oral histories of early Board members and students?

Each of those eight or ten cognitive processes has become an issue in our longer methodological analysis in our project. They seem very important for thinking about the relationships between historical and ethnographic methods, at the level of a concrete practical activity of inquiry. Later, I found historians such as Gottschalk (1944) presenting examples of similar inferential thinking from documents.

In addition, finding and working for two years on the Minutes of the Board of Education has been a most dramatic and emotional research experience for me, second only to my original experience as participant observer in Geoffrey's class almost two decades ago (The Complexities of an Urban Classroom). The massive and relentless month in, month out, year in, year out quality, the written at the moment and not to be changed record regardless of what happened later quality, and the moved, seconded, and passed quality focusing on decision making provided a "reality" against which all other data could be compared, contrasted, and triangulated.

Beyond those substantive intellectual processes, the personal consequences of this discovery of the Minutes seemed multiple. First,

an energizing, exciting quality appeared. Motivation ran high. Day after day I returned to the new kind or genre of data for the surprises it contained both substantively and methodologically.

A related, but second aspect occurred. Creative thought was stimulated to high degree. I was in a welter of new data, new formats, forms, or categories of data, new precepts and images, new items about which choices had to be made, and new construals or perspectives formulated. Continuously I asked "How do I cope with all this?". The materials were manifestly relevant, for little items kept appearing here and there and seemed to be connected? What were the patterns and structure of ideas which would encompass all this in some way relevant to our original problem and the way that too was evolving? The data transformed the problem from "A fifteen year follow-up of an innovative school and its faculty" to what we now call, "Innovation and Change in American Education", a very different agenda. And I was struck with the parallels to changes that occur in problem definitions as creative artists work (Beittel, 1973).

Along the way came the doubts about the degree of or levels of creativity--is it new to oneself versus is it new to the field? Were we just rediscovering the wheel? Tours occurred into the nature of history, the philosophy of history, the methods of history. Similar trips occurred into the substance of educational history--local, state, and national. Even book titles with a ring of the history of curriculum, teaching or schooling in England, Australia, and elsewhere caught my eye and dollar--or pound--in the book stores. Conversations occurred with my historically trained colleagues. They were assaulted

with concrete particular items and with abstract and general items. What do you make of this? What do you think of that? What's the conventional historical wisdom on this, on that? And on and on.

When the energizing aspect collided with the creative aspect, the turmoil raced through all parts of my life. It got in the way of my teaching, although sometimes, it blended in neatly, effortlessly, and successfully. It made me impatient with committee meetings and organizational maintenance activities. It led to multiple new kinds of readings as I indicated in this instance, further histories, biographies and autobiographies of historians, and books on historical method and philosophy of history. It awakened me in the middle of the night, impatient to get some new idea down, yet unhappy that if sleep didn't come, I would be too tired to push on for more than a couple of hours the next day. It meant drinking too much coffee and eating too much to dispell the sourness. It meant the need for heavy doses of exercise--handball, walking, gardening to tone down the mania and to take the tension out of my arms, legs, and head. As I said, it was an incredibly important all encompassing professional experience.

2.3 The Occasional Rare Document

A number of historians we have read have talked about the joy and excitement in finding a key document which made a point, unraveled a knotty problem, or illuminated an enigma. This happened to us on several occasions; but the prototypical incident concerned the genesis of Milford's Policy Handbook in the early 1960's. We had encountered several years of minutes between 1962 and 1965 and realized that Dr. Steven Spanman, Superintendent between 1962 and 1966, had initiated the changes. No one, in Milford or at Washington University with whom I

talked knew about the general intellectual origins of the particular system. The format seemed too complex and too systematic to have come whole cloth out of his head. It looked like too large an effort for any local district to have created. Then one day I ran into a Central Office staff member who had been around in those years and who tended to be a bit of a "pack rat" in terms of keeping things. She disappeared into an old file drawer and brought out "the document." It was a mimeographed copy of the "Southern City Public Schools Policy Procedures." Southern City District was Spanman's prior work place. Scrawled all over it were Spanman's deletions and additions to alter it into a Milford Public Schools document. Rather than being addressed to Southern City's principals' meeting it was altered to a Milford Board agenda item. To become Milford District policy it required a Board vote for adoption.

The "introduction" to the original report gave a little history of Southern City and the original researchers and consultants, Davies and Brickell. The first section dealt with administrative "organization", who would do the reworking and implementation. A second section indicated "steps and procedures". A third contained "materials to be supplied." The first appendix contained the eight "policy study committees" by title and personnel involved. The second appendix contained the "policy classifications and codes" over the eight areas. Each page contained the minimal changes to fit Milford.

And there it was. My immediate emotional reactions were "Oh, my god", "I can't believe it", and "ecstasy". Methodologically, it was the lynch pin, the ribbon around, the string that pulled it all together. It had the specificity that tied all the inferences down. It answered

all the questions. I got it from an individual directly involved with it twenty years before. Substantively it led us to Figure 1, Origins of Milford's Policy Handbook. Three strands of activity came together: a strand from Southern City, a research and commercial strand from the work of Dan Davies and William Brickell, and a strand from Milford. When Spanman joined the Milford School District, he brought a resolution of a long standing problem which had been a source of conflict between the Board and the prior Superintendent, McBride. The nature and format of the policy handbook is such that it has remained, with revisions, in the District every since. The meaning and implications of the vignette for a variety of problems such as policy formation, educational innovation and change, relationship of research, theory and practice, macro and micro levels of analysis seem open for analyses. This was history and ethnography at their best.

Insert Figure 1 About Here

2.4 Triangulation Possibilities Between History and Ethnography

It's a minor matter, at one level, but critical at another level. Dr. George, the current Milford Superintendent, gave us total access to the various sets of public records--the Bulletin to district patrons and the School Board Minutes. The latter exist in bound volumes and are stored in a locked closet just off the Superintendent's office. The minor point: we never took any of these out of the Central Office building. Concretely, we kept the two or three volumes with which we were working on top of filing cabinets in the secretary's alcove. We left the volumes there over night which gave us simple access, early in

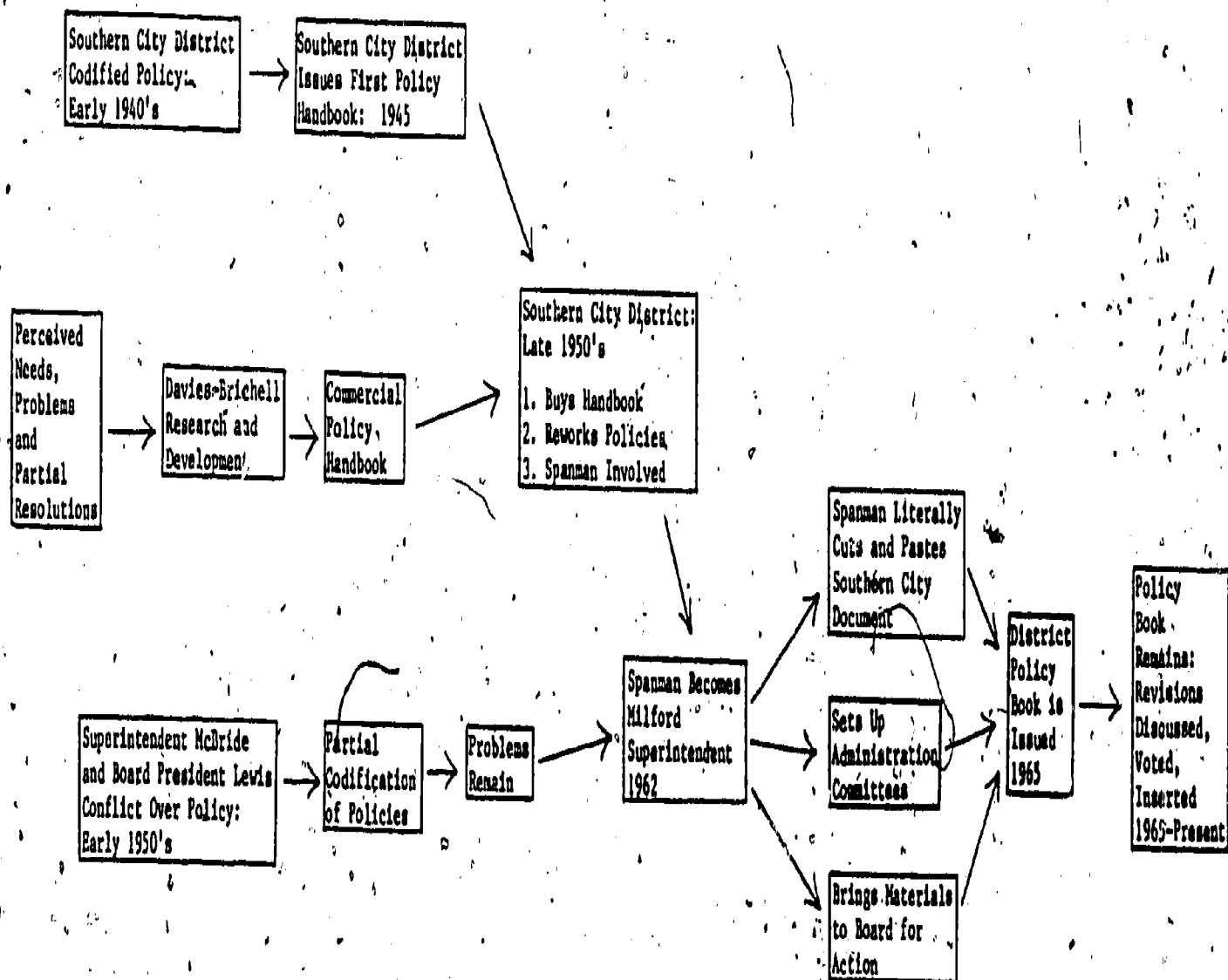


Figure 1: Origins of Milford's Policy Handbook

the morning or late in the afternoon whenever we came up to work. When the Superintendent and secretary were off on vacation, we had access through one of the assistant superintendents.

The major point, however, is that by working in the Central Office building, actually in a small conference room off the main hall, we gradually came to meet most of the secretaries, clerks and Central Office professional staff. They came to know the project in two forms, "the return to the Kensington School" and a "history of Milford." The most general reaction was amazement at the long involvement in reading minutes--off and on for two years. With the legitimization of the Superintendent and through our own efforts to spell out our activities, which included sharing the research proposal early and preliminary documents later, we gradually built a relationship that merged helping, friendship, and professional collegiality. These concrete activities and developing relationships set the occasion for some of the most striking instances I have encountered of triangulation or multi-method approaches to data and ideas (Smith, 1979). At this point, historical method and anthropological method merged into a general open ended set of field methods.

Figure 2 illustrates the half dozen classes of data and activities. First are the bound volumes of Board Minutes themselves. I don't really know much about other districts and their Minutes and how they are similar or different, but Milford's volumes are fascinatingly diverse. In one form or another, these Milford District records go back to 1914.

Insert Figure 2 About Here

1. Multiple items bound into the Board Minutes
2. Other Central Office documents: historical and contemporary
3. Creation of Data Files to our questions and inquiries
4. M.A. and Ph.D. Papers--Inside and outside Milford
5. Short term conversations and long term vivid memories
6. Other contacts and oral histories
7. Intensive interviews

Figure 2: Classes of Triangulated Data and Data Gathering Activities

Beginning in the 1930's, each year is bound separately; before that the years are clustered. In terms of triangulation and multi-methods one of the most important aspects of these records is that they contain other kinds of documents beyond the minutes themselves. This fact seems to make them a much more powerful set of records. For instance, at different times they included:

- 1) Notes of other meetings
- 2) Original copies of letters to and from the Board or Superintendent to citizens, government agencies, etc.
- 3) Financial records: bills paid, budgets, bank loans
- 4) Superintendent's agendas
- 5) Reports on school issues by internal staff and outside consultants
- 6) On several occasions stenotyped records (50-90 pages) of meetings involving severe conflict between staff and between (and within) the Board.

In short, the Minutes were much more than minutes, they were multiple kinds of primary documents with curious kinds of independencies and interdependencies, open to triangulation.

While working on the Minutes, a second kind of triangulation occurred. It blended document analysis with brief interviews. One of the most vivid of these is what I've called my "Nussbaum story". It goes like this. In reading the Minutes of the late 1950's a conflict arose in which the Board voted not to rehire a social studies teacher, Mr. Nussbaum. Allegedly he was having discipline problems and was too familiar with some of his students. He argued that they were firing him because he had distributed some literature on the teachers' union. The Minutes summarized the controversy as well as contained some of the items in the controversy. Two of the items did not fit well with the Board's allegations: Nussbaum was in his seventh year of teaching and

also he was chairman of the high school social studies department. Problems in teacher student relations--discipline and familiarity--are not problems typically of experienced teachers and department chairpersons. In the vernacular--things didn't smell right. It was at this point, the triangulation occurred. One of the Central Office staff wandered by the conference room in which I was working, as he was prone to do, and stuck his head in to say "hello." After the greeting, in my best non directive style, I said, "Say Bill, did you ever know a teacher named Nussbaum?" His response was immediate, direct, and vivid. "Oh, yes, the Board and the Superintendent were really on his case for union activities." Then he proceeded to tell me an even more devastating story. It went like this. "At the time, I was teaching fifth grade at Milford Village School. The Community Teachers Association (CTA) President was teaching sixth grade in the room next to mine. One afternoon, the Superintendent, the high school principal and our principal all descended on him about the upcoming CTA meeting. He was told in no uncertain terms that he was to set the agenda beforehand, to have no additional items added, and under no circumstances was Nussbaum to get on the agenda. He couldn't sleep for three days. Later Nussbaum heard about it, called him, and said he wasn't going to raise a fuss at the meeting."

The triangulating point of the story is obvious. Mixed data in the documents, incongruency with my knowledge of schooling, a Central Office staff member's immediate reaction, and then the very powerful story of teachers teaching together and the trauma faced by one's friend presented a powerful view of the events.

Instances of triangulation occurred with each of the other items in Figure 2. Other dusty documents appeared off of shelves, M.A. and Ph.D. papers were loaned, in some instances the District began to accumulate data files in areas where we were searching for clear records, and the network of contacts grew and grew. As we have indicated, ethnography and history merged into a series of efforts to find credible data and to build a synthesis or view of the school.

2.5 Eliminating One's Contextual Ignorance and Imagining One's Self Into The Past

Being new to historical research and coming at the task from our idiosyncratic brand of ethnography, left me with a continuing problem of "eliminating one's contextual ignorance", and "imagining one's self back into an historical period." The phenomenon arose this way. In two separate interviews, one with a teacher who had been in the District a number of years and one with a former student of the Class of 1931, items were raised which we fumbled with because we did not know enough of the history of Milford and of Suburban County. For instance, with Mrs. Irma Hall, a Kensington teacher, we didn't realize that the Milford District and the Marquette District had merged as late as 1949 and that those two schools were the elementary school program when she joined the faculty. The interview put us on to some of the issues, which we've since explored, but we did not raise key items in that early history. Similarly, in talking with and later reading through the interviews with Mr. Elbrecht, from the early years of Milford High School, we did not have a view of the west end of Suburban County and Milford's relationship to the numerous other small three-director districts. Nor did we know much about Suburban County in general.

These "fumbles" got us started reading contemporary local histories and records from the turn of the century. Each source, as is true in all our academic work, tends to lead to several more, and in a few hours of searching one has three months of reading and a half dozen new problems. For example, we believe a need exists for a history of Black education in Suburban County. Similarly we believe a need exists for a biography of a Suburban County Superintendent who served for thirty-seven years in that office, an incredibly long period in an era of incredible change. Each of these would clarify important aspects of schooling in Milford since the turn of the century.

As I have said, for an individual with minimal training in history, and with no training in historical research methods, one of the most difficult problems has been "imagining oneself back into an historical period." No images of Midwest State in 1870 or Suburban County in 1920 came to mind. The hurly burly of the rapid county growth in the post war years clouds visions of an earlier more rural and small town state of affairs in the state and county.

The two channels which seemed most helpful and fruitful in developing images were two of the main sources of data. As I indicated, open ended, oral history type interviews with older residents of the community and professional staff, filled in gaps regarding the coming of roads, electricity, volunteer fire departments and so forth. In addition, I started reading histories of all kinds. First, were United States histories and particularly the splendid three volume set by Boorstin (1978), The Americas. Second, were books on the history of American Education, e.g. Cremin (1980) and Butts (1978). Third, a variety of local histories appeared. A folksy history of a Suburban

County school district seemed very much like the Clear Valley School before Milford became Milford. Histories of "Big City", its neighborhoods, its schools, its politics were sought. A centennial history of Midwest State University, and especially its College of Education provided another look at the total state educational establishment, the training of teachers, and the beginnings of educational research.

Unless our label, "eliminating one's contextual ignorance" be taken too negatively, a few positive aspects seem latent in the approach. It follows the general inductive, interactive mode of our general research style. One learns a little here and a little there and those items play into each other and into later interviews, documents, reading, analysis, and writing. This is tremendously motivating, in the sense, one's always discovering something new, patterns emerge, flow, and stabilize. It's very exciting intellectually, it keeps one at the task, hour after hour, day after day, and month after month.

The particular configurations from established interpreters as they appear in theoretical structures and in accepted descriptive formulations do not become overly potent filters as the new information is acquired or processed. At some point, what Hexter calls "the second record", what the historian carries about in his/her head from one's general reading and knowledge must come into play. As we have argued elsewhere (Smith, 1979 and 1980 and Smith and Dwyer, 1979) eventually that second record has to be well filled out and articulated with the specific problem under study and with the data being developed. We're arguing that "too much too early" may be stifling to one's imaginative construing and reconstruing of the problem and the analysis. Unless

it's eventually done, one may find one's only rediscovered the wheel, and probably a lopsided one at that. Even so, in the process, one keeps eliminating one's contextual ignorance.

Several substantive outcomes of this activity seem most critical. We were struck by the recency and the magnitude of educational changes. The basic change in Suburban County from ninety school districts to about two dozen in Suburban County happened since World War II. This was less than a dozen and a half years before Spanman was to build a space age open school. In the mid 1960's when we first came to the District and did Anatomy, that recency didn't even occur to us. Second, the "catastrophic" current changes--race, sex, declining enrollments seem less catastrophic when viewed from this longer perspective. Periods of high change, growth, complexity and specialization, as we have argued substantively elsewhere, have been lived through, thought about, and fought about (Smith, et. al. 1982). The current changes, modes of reaction, and adaptations will become later chapters in someone else's historical account. This is neither to argue for a "polyannish progress" nor for a "nihilistic doomsday." Nor to argue for change, reform, or innovation coming too fast (for some individuals and groups) or too slow (for other individuals and groups). But rather to indicate that much of the hopes, aspirations, conflicts, disagreements, and action have had their counterpart in prior times and places.

Finally, and a point that first arose when William Geoffrey and I were studying his classroom at the Washington School, is the realization that his world of urban education, at the classroom level and at the school level, had its own kind of patterns and order. And that this order and pattern could be observed, analyzed, reflected upon, lived

with, and in some instances changed. This seemed a most important insight then. Now it seems that earlier historical times, places, events, and people could similarly be ordered and patterned. This seems an important part of "imagining oneself back into an historical period."

2.6 On Knowing How Things Turn Out

As we began some of our historical meanderings in the multiple kinds of records and data which exist in and around Milford, another issue or two left me a bit edgy. With our 1971 book, Anatomy, and with the ethnography of the 1979-80 year we knew beforehand how the story and some of the sub stories came out. In a sense, we had some of the themes defined ahead of time and our search of the records was a clarification of the origins and development of those themes. Racial issues, the neighborhood school concept, demographic changes, the role of the Federal government, the nature of the curriculum, the importance of Superintendent Spanman, the issues around buildings and physical facilities suggest some of the themes from our contemporaneous studies. Somehow we seemed to be in a game different from the positivistic prediction and control social science I had grown up with. At the time, it left me with a question of the legitimacy of the whole enterprise.

Also, for a long time, we had been in debates with various colleagues over the importance of narratives, portrayals, and stories, e.g. Stake's (1977) view of evaluation versus the importance of generating grounded theory, eg. Glaser and Strauss' (1967) extreme position. We tended to follow George Homans (1950) and argued strongly for both (Smith and Pohlman, 1976; Smith, 1979; Smith and Dwyer, 1980; and Smith, Dwyer, and Prunty, 1981).

One of our recent joyful experiences has been to find that the historians split on the same kind of issues. For me, Hexter's book, The History Primer, broke open the narrative/analysis dilemma by offering a third alternative. He took some of the edginess out of knowing the outcome. He presents an illustration of historical writing, an American baseball game, which has received considerable notoriety among sports buffs. American baseball is one of the most documented aspects of American life. Records exist on the details of every player, every game in the season, and of every aspect within the game--hits, runs, errors, and so forth. Newspapers chronicle daily each game. Daily, weekly, and monthly papers and magazines offer commentary. For "important" games, as in the final game of the 1951 National League season, the commentary is unending. This is the game Hexter, the historian, chose to write about as an illustration of history with a small "h", the mundane and unimportant. In writing his history he ignored large masses of available factual data, omitted reference to star players who were unimportant that day and selected threads and incidents that culminated in the high drama of the game, Bobby Thompson's home run in the ninth inning. This example of what he calls "processive explanation in history," a form of explanation, is an answer to the "why" question which has plagued historians. He comments in summary:

It is this outcome that warrants the substance, the structure, and the tone of the introductory section of "The Last Game." In other words, the outcome defines the appropriate historical macrorhetoric; and the macrorhetoric in turn dictates the selection of "facts" or more accurately data to be drawn from the record. Or to put it more bluntly, amid a mass of true facts about the past, too ample to set down, historians choose not merely on logical grounds but on the basis of appropriate rhetorical strategies. (1971, p. 190)

In effect, he is making a case for the importance of rhetorical principles as part of the historian's craft. In the course of his analyses he raises concepts such as record, focal center, event clusters, fragments, promissory notes, judicious omission, universe of discourse, pivot points, and unstable active explanandum. Although he would probably disown the label, he has a "theory" of historical method. All this we found helpful in thinking about what we were doing in our Kensington Revisited project. Processive explanation gave us a rationale for what we had been doing in a social science which knew the outcomes. It bridged history and ethnography in a way which cuts to the heart of the logic of social science, the concept of explanation.

3. The Merging of Methods and Theory on Ethnography and History

At this point, I want to back off a bit from the day to day data based generalizations of our activities that involved ethnography and history in our Kensington Revisited project. With this detachment, comes several broader ideas concerning the relationships among history, ethnography, and the cumulating aspects of our work. Briefly, these include 1) diversity within ethnography and history, 2) some similarities and differences, 3) a concern for process as a mode of integration, and 4) a move to broader assumptions, metatheory, or paradigms.

3.1 Diversity Within Ethnography and History

As I began to ground our activities further in the methodological writings of historians and anthropologists, I found myself in a quagmire of diverse opinions. I had known the anthropologists did not agree with one another. In trying to write a general essay on ethnography a year

ago, I was struck by the theoretical differences among such major figures as Bronislaw Malinowski, William Foote Whyte, and Clifford Geertz. The differences among functional, social interactional, and interpretive anthropology suggest that the "real" problems in resolving their diversity lay in their assumptions, their metatheory, their underlying paradigm (Smith, 1982).

Among historians, similar differences exist. Morton White, an American social historian (1963) who is keen on narrative history is a logical positivist seeking broad social laws. Jack Hexter, an American who specializes in British history wrote, as indicated earlier, a book called The History Primer which is a long argument with Carl Hempel's (1942) classical paper, "The function of general laws in history". Lawrence Stone, an Englishman, trained at Oxford, and now at Princeton, has published a recent collection of methodological essays and reviews under the title, The Past and The Present (1981). The first, "History and the social sciences in the twentieth century" is a cautionary note to the historians. He feels the logic of the social sciences is in real trouble and they, the historians, should be wary of their borrowings from social science. The second essay is entitled "Prosopography" and refers to collective biography or multiple career line analysis. He raises a set of issues undergirding our study of the original Kensington faculty. The third he calls "The revival of the narrative: reflections on a new old history." His new historians, after a flirtation with positivistic social science, quantification, and general laws are returning to narratives "directed by some 'pregnant principle'", which sounds a good bit like Hexter's processive history. Further, he argues

they are more apt to look to the interpretive anthropologists (such as Geertz and his thick description) as the more important kind of social science for stimulation.

Reading these materials, by eminent anthropologists and historians suggests several tentative conclusions. 1) Intellectual ferment and turmoil is everywhere. 2) Guidance from other disciplines for educational theorists, researchers, and practitioners is not going to be a simple "1 to 1" and "2 to 2" kind of borrowing. 3) Synthesis and integration, to whatever degree it occurs seems to involve a next level higher set of abstractions, a metatheory or paradigmatic level of discussion. 4) It probably should be left to philosophy, but I'm reminded of Kaplan's (1964) argument of scientific autonomy. The working scientists, the crafts people of an area, must put their own intellectual house in order on their way to solving their own particular substantive problems. If one is looking for distal goals that seems a worthy one.

3.2 Some Similarities and Differences

Rather than tackle directly this array of anthropological and historical methodologists, I have borrowed bits and pieces to illuminate some of the procedures we used and the ideas we developed as we tried to carry out and understand aspects of our Kensington Revisited project. Some of these have been noted along the way. The fuller essay is the last volume, the methodological appendix, of that series. For the moment, I find the diversity and pluralism among historians and anthropologists helpful in suggesting ways to do the inquiry and in

giving ideas toward the rationalization of what one does. But the diversity is frustrating in not providing a logical structure of criteria for judging proposals and products.

As indicated in the introduction, one of my chief guiding hypotheses of the differences between ethnography and history was the dependence of the historian on data fragments. Now, especially after reading Gottschalk's (1945) essay on "The historian and the historical document", I believe the hypothesis remains essentially true but that it is a matter of degree. The ethnographer also deals in data fragments, but usually he has a chance at larger and more relevant chunks.

A second difference seemed to exist in the "active huntsman" role of the ethnographer, so eloquently captured by Malinowski:

But the Ethnographer has not only to spread his nets in the right place, and wait for what will fall into them. He must be an active huntsman, and drive his quarry into them and follow it up to its most inaccessible lairs. (1922, p. 8)

Again it seems to me that it is a matter of degree; there are historians and historians. The data oriented kind chase the aforementioned "fragments" in varied, creative, and persevering ways.

A third difference, and one that now seems to me to be much more critical, is what lies behind the cliché of the ethnographer as the research instrument. In the historian's terms this has to do with "primary" and "secondary" sources. For the historian a primary source is "the testimony of an eyewitness." A secondary source is "the testimony of anyone who is not an eyewitness--that is, of one who was not present at the event of which he tells" (Gottschalk, 1945, p. 11). In this framework the ethnographer who observes is producing his own

primary sources. He is the witness to the event. From the historian's point of view that has to be a powerful and important difference.

Although the ethnographer still deals in fragments and although the ethnographer is a more active huntsman, it is the production of an eyewitness account by the researcher which is the devastating difference. Somehow I had never quite phrased it that way. But that in turn produces complications, for the eyewitness is also the "detached" story teller and analyst. Does he lose more than he wins?

Within his discussion of the historian, Gottschalk is very precise on the need to examine each document in terms of the "particulars", each item in any document. Some may be based on eyewitness testimony, other items may be secondary, information that has come to the document writer from someone else. Again, to think of ethnographic interviewees and informants from this perspective casts new meanings on them and their reports. From a psychological perspective, I'm reminded of the relationships between overall test validity and item analysis and item validity.

Note here also the contrast of the ethnographer who is an eyewitness observer and the ethnographer who uses an informant. The latter seems more like an historian. Similarly the ethnographer, or case study researcher, who relies on interviews is also more like the historian, and perhaps identical to the oral historian. Further, if one must use an interpreter or translator, one puts another item between the event and the eyewitness. The commonalities keep suggesting ways to interpret the activities of each group of researchers.

But even here, the issue turns complicated, as the ethnographer is apt to say to the historian. If "the ideas, feelings, meanings of the

participants" are the "real data" as some more interpretive ethnographers and case study researchers argue, then the words of the interviewees might be "more primary" than the observing ethnographer's eyewitness reports. This is complicated in at least two further ways. The observer, insofar as he is a participant observer, can observe and report on his own internal states thereby producing another kind of primary document. And, secondly, at least since Freud, there is theory and data to suggest that an outside observation of slips of the tongue and unintentional mistakes may have a kind of validity that self reports do not have.

Once the limits of the data are held constant, the historian and the ethnographer seem to be carrying out the same intellectual activity when the ethnographer does his descriptive narrative and the historian does his historiography, which Gottschalk defines as "the imaginative reconstruction of the past".

In brief, and in spite of the pluralism and variety, reading historical methodologists has been a profitable exercise in rethinking the nature of what one does as an educational ethnographer.

3.3 Process as a Mode of Integration

One's origins seem to both help and hinder one's inquiry and thought. I came to ethnographic research from a measurement oriented kind of psychology. One of the texts I taught from was Remmers and Gage's Educational Measurement and Evaluation (1955). One of the parts of that book which I liked, as an educational psychologist, was the unit on measuring the environment, including teaching and the classroom. About the same time I was enthralled with Cornell, Lindvahl, and Saupe's An exploratory measurement of individualities of schools and classrooms

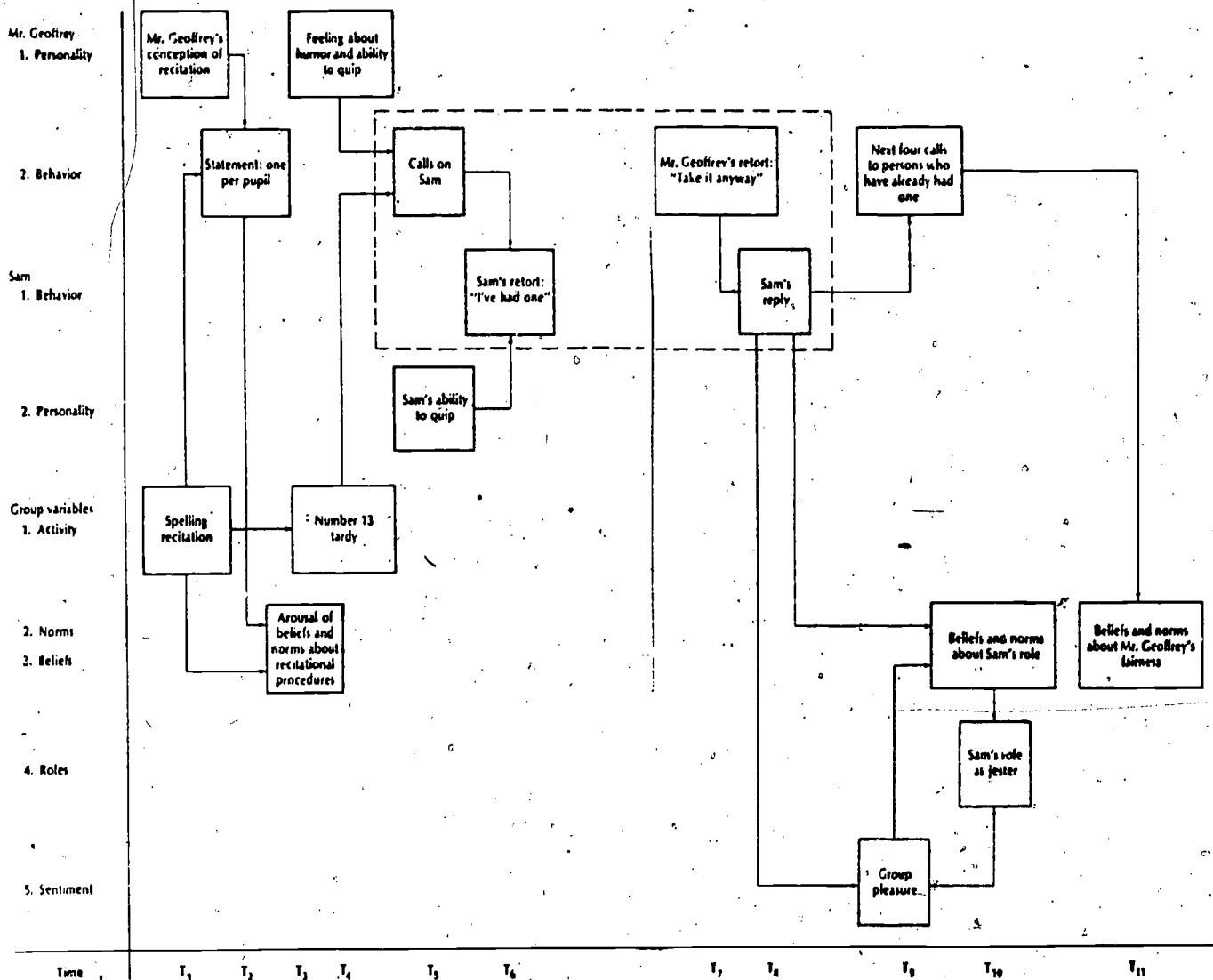
(1953), as well as H. H. Anderson's dominative and integrative teacher personalities (1945) and Withall's (1948) classroom climate index. All this was pre Ned Flanders. Bryce Hudgins and I put a lot of this into our Educational Psychology text (1964). To me, one of the problems with this approach was that it was static, cross sectional, structured. It was time free. The larger problems in teaching, it seemed, were processual, sequential, longitudinal. Ethnography, or participant observation as we tended to call it, or the micro ethnography of the classroom as Fred Strodbeck labeled it, seemed to put us next to the evolving and developing classroom over the semester. It permitted a view of the teacher and children as active, interacting human beings. This was one of the important lessons, for me, from my work with Geoffrey in The Complexities of an Urban Classroom.

We struggled mightily to produce models such as Figure 3, "A process analyses of an interactional episode." It was a vehicle, we thought then, useful for teachers to think about their classrooms. It enabled us to begin to think about teacher decision making and planning, a very different kind of view of human beings than our non-intentional educational psychology colleagues (eg., Medley and Mitzel, 1958, 1963) were assuming and very different than the operant/respondent theories some learning psychologists were arguing. The idea was simple. On the abscissa of the figure is a time line. The ordinate is a teacher with a personality who engages in certain behaviors and actions. There are pupils, like Sam, who have personalities also and who engage in behaviors and actions. As these actions become interactions of various sorts (eg., banter, personalized interaction), a social system of roles, sentiments, beliefs and norms develops. All that seems a part of Figure 3.

Over the years and through several projects we struggled with the model. That struggle achieved a slightly larger generality in our methodological essay, Go, Bug, Go, as Pat Brock and I argued about units for classroom analyses and classroom processes useful in thinking about her general science class (Smith and Brock, 1970, Brock and Smith, 1981).

The most significant addition to our thinking occurred as we were trying to order our data from the Kensington Revisited project. There we began talking about "a longitudinal nested systems model of educational innovation and change." We had gone back to the Kensington School to see the longer term fate of an educational innovation, a specific planned change, in the Milford School District. We found the school different in a number of respects. One of the most obvious was a shift in the pupil population from 100% White to 60% Black over the fifteen year period. One of the obvious first answers to why this had happened was the United States Supreme Court decision in Brown versus Topeka in 1954. Before this time, Midwest State law made it illegal for Black children and White children to be educated in the same schools. Without that decision, or an equivalent set of changes, the school would not have been different in this regard.

The general model we present is a simple grid. The nested systems are arranged hierarchically on the ordinate; the time line was constructed on the abscissa. The generic quality of the model is suggested by the possibility of inserting any set of nested systems on



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Figure 3: A Process Analysis of an Interactional Episode Between Mr. Geoffrey and Sam and its Implications (Smith and Geoffrey, 1965, pp. 56-7)

the vertical axis and any time line on the horizontal. In Figure 4 we insert the systems we have seen as relevant to Kensington and Milford and add a time line from 1910 when the first records of Milford appear. Into this we place some of the items and events from the stories we have told in our narrative. Simply, we are capturing instances of innovation and change, putting them into categories that are more general and abstract, and then arranging them to demonstrate their temporal relations. We contend that each such conceptual act adds clarity and depth to the Kensington story and improves our ability to think about our original problem: What happened at Kensington? The process has not only been enlightening in this respect but has expanded the initial conceptualization of the study by suggesting further fruitful avenues of inquiry. For example, as we view the Milford District story as an important influence on Kensington events, that story becomes significant in its own right. Now one of our guiding questions is not so much "How and why did this school change from 1964 to 1979?" but also "Why did the Kensington School appear at all in the Milford School District?" Elsewhere we have tried to answer that substantive question (Smith, et al 1981, 1982).

Insert Figure 4 About Here

When we bring the model into specific focus on the changes in the Kensington School we find a series of events, which we have drawn perhaps a bit too linearly, but which capture the flavor of international, national, and state changes influencing the community which in turn influences the Milford District. Kensington's changes,

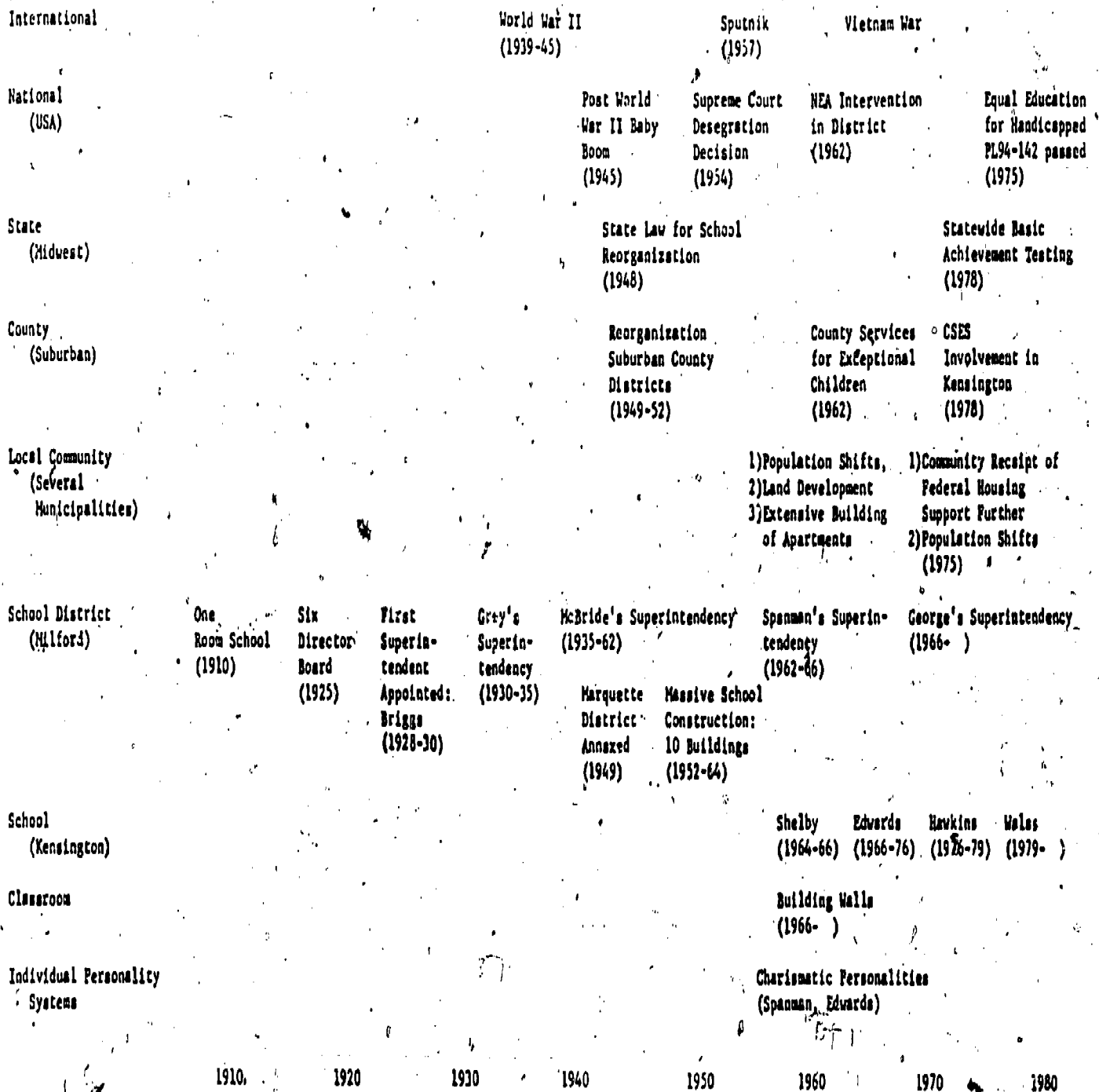


Figure 4: "Selected Events and School Personnel Arrayed on the Longitudinal Nested Systems Model (Smith, et al. 1981)

"its reversion to the old Milford type" as we predicted a decade ago involve a complex set of demographic changes, perceived pupil changes, and curriculum and instructional changes. Figure 5 specifies the items from our analysis.

Insert Figure 5 About Here

This perspective has implications for both the metatheoretical and the theoretical levels of analysis. For example, it argues implicitly for a contextualist root metaphor rather than a formistic, mechanistic, or organic one (Pepper, 1942; Sarbin, 1977). Conceptually, it seems open to varied substantive theories, for example, organizational, political, or cultural.

Further, the model helps locate our approach in relation to other social science studies of change and innovation in education. First, we find ourselves examining increasingly longer periods of time for relevant information in our inquiries. This differs from the snapshot variety of study that examines a brief, specific period. Second, our perspective involves a holistic view of events; we contend that one cannot understand an innovation or change in a system without considering the larger systems of which it is a part. Third, our model makes explicit a hierarchic arrangement among the nested systems. It highlights the direct and indirect "controls" one system may impose on another. Fourth, the longitudinal nested systems notion allows one to focus on parameters or "givens" of the field of action set by one system upon another. Fifth, it assumes some autonomy both analytically and practically for each system--perhaps less than some educational

INTERNATIONAL

- 1) Rise and fall of population e.g. postwar baby boom
- 2) Inflation

NATIONAL

- 1) Brown vs. Topeka, 1954
- 2) Federal Housing Subsidy Program
- 3) EDC Regulations
- 4) Public Law 94-142

STATE

- 1) Mandated use of Basic Skills Test
- 2) State guidelines on discipline

COUNTY

- 1) Land development
- 2) Apartment construction

- 1) Constant population turnover

- 2) Increase in minority population
- 3) Increased concern with discipline
- 4) Back to Basics Ideology
- 5) Reinvestment in conservative boards of education
- 6) Resistance to increased taxes
- 7) Increased frequency of single parent families
- 8) Drop in mean I.Q.S.

- 1) Administrative turnover
- 2) Re-establishment of "old boy" leadership group

- 3) Redirection of school boundaries
- 4) Strong discipline orientation
- 5) Termination of innovative plans
- 6) Declining financial resources

Demographic Changes at Kensington

- 1) Racial changes
- 2) Socio-economic status changes
- 3) Increased frequency of single parent families
- 4) "In and out" pupil turnover
- 5) Increased diversity of pupils

Perceived Pupil Changes at Kensington

- 1) Perceived discipline and behavior problem
- 2) Perceived attitude problem
- 3) Perceived ability and achievement decrements

Curricular and Instructional Changes at Kensington

- 1) Walls added on original plant to create smaller more traditional instructional classroom spaces
- 2) Graded curriculum levels
- 3) Textbook directs more traditional instructional approach
- 4) Extensive remedial and special education programs
- 5) Corporal punishment and after school detention programs

COMMUNITY

SCHOOL DISTRICT

KENSINGTON SCHOOL
"RESPONSE TO THE 'OLD KENYON TYPE'"

Figure 5: A Longitudinal Nested Systems Portrayal of the Changes
in the Kensington School
(Smith et al. 1981)

theorists imply and more than some educational practitioners perceive. Sixth, it builds upon a psychology of individual actors, involved in events or scenes, that cumulate into meaningful structures resembling plots in drama and literature (Kelly, 1955; Sarbin, 1977). Seventh, it includes a respect for the chance event, the fortuitous, the serendipic, which nature forces upon us in the form of health or illness, death, and luck or natural disaster. Finally, and our major point here, the conception aligns closely with the perspective of some historians. We differ from some of them, too, in that our longitudinal approach carries the time line to the present, the realm of contemporary events, and sets the stage for Hexter's perspective on processive history.

In effect, what started out as an ethnographic study of a school became something very different. In part, it became an historical study of a school district. As we struggled to order this world we ended up with a process type model which has close linkages in form and purpose with our earlier analyses of classroom interaction.

Partly we feel our kind of educational ethnography has had a strong process orientation. It seemed to fit some of the early interactionally oriented applied anthropologists such as Arensberg and MacGregor (1942), Arensberg and Kimball (1940), Chapple and Arensberg (1940) and Whyte (1955). However, it seems to fit less well some of the mainstream anthropologists in the functional/structural schools. When we turned to "cultures through time" we found so far only a few individuals speaking to the issue. Radcliffe-Brown's (1958) account raised synchronic analysis and diachronic analysis but mostly argued the difficulties in the latter. Vansina (1973) and Haekel (1973) present images of the diachronic modes intertwined with historical references.

At this point in our work, I believe the diachronic or process mode is an appropriate and powerful way to join ethnographic and historical modes of inquiry. It handles well a cluster of important phenomena in the micro world of Geoffrey's classrooms and in the macro world of the Milford School District. Any conceptual tool that bridges events as diverse as Sam's jester role and Kensington's transformation from an innovative school to a traditional school is a powerful addition to our ways of thinking about schooling.

In this essay, I have not taken up the process analyses of individuals and the data we have on the careers and lives of our original Kensington faculty. The interplay of life history, biography, and autobiography with historical and ethnographic method is vivid, striking and informative. We will have more to say about that on another day. It seems to clearly belong in the discussion at this point.

3.4 The Move to Broader Assumptions, Metatheory, or Paradigms

A second kind of serendipity also seemed to be in the offing. As our story telling and analysis was moving to a close, we found, lurking in the background, concerns that we have often labeled "broader assumptions", "metatheoretical issues", or "the nature of one's basic paradigm." Somehow we wanted a next level of consistency, which never seemed attainable, for it was too big a problem. In field work, at least as we practice it, the intensive literature search often comes relatively late in the inquiry process. On those occasions we use it to help see our data and ideas in a broader, intellectual setting. We try to generalize our findings. In part, this intellectual activity is the relation of the particular instance to a larger, more general, more

abstract class of events (Smith, 1979; Diesing, 1971). This becomes a practical task when one intersperses ideas and stories or when one views conclusion sections as offering the analyst and interpreter a variety of intellectual options.

As we were finishing a major piece of the Kensington Revisited project, what we called "Milford's recent history: the school district as contemporary context of the Kensington School," and as we were doing our "literature survey," we were brought up short. We found ourselves stunned in reading Keith Goldhammer's provocative little book, The School Board (1964). We remarked to ourselves that our account of Milford's recent history, heavily an account of the Board and Superintendents, was quite different. In trying to isolate those differences we generated a list of a baker's dozen items. And, as we looked at that list, we felt that our tacit knowledge had been running well out ahead of our formalized knowledge--we knew better than we realized, a not unusual phenomenon in this kind of research, at least as we practice it. Consequently, we present Figure 6.

Insert Figure 6 About Here

Obviously, our intent is not to disparage Goldhammer's work, which is provocative in its brevity and clarity, but also obviously, we believe we have come out of our experience with a point of view that is quite different. We believe the point of view is not simply different in the sense of a substantive middle range theory of school boards, but that it has the potential for an integrated view of schooling across several levels of analysis. For instance, it encompasses the kinds of

1. full of people with interests, motives, and sentiments
2. who are making choices and decisions
3. which lead to actions and interactions
4. which are in a context--historical and contemporaneous
5. which gives a dynamic or processual quality
6. as a case it is interrelated and systemic
7. substantive focus is on innovation and change
8. blends/mixes/integrates the specific/concrete/particular and the general/abstract/universal
9. focuses on the "real", the "is" rather than an external "ideal", on the "ought", or the prescriptive
10. part of the real is the "multiple ideals of the several actors and subgroups out there"
11. those ideals are often in conflict and are resolved by a variety of social/political processes
12. eventually all individuals and subgroups make up their own minds
13. ultimately integrates is/ought dichotomies in an R. N. Hare (1952) type configured decision of principle
14. as personal experiences cumulate (and case studies are surrogates for those) one builds toward one's own syntheses of decisions of principle

Figure 6: Contrasts With Goldhammer:
Toward a New Perspective

of data one collects when one thinks about schools, the kinds of methods and procedures linked to those data, the kinds of accounts one renders of those phenomena--both common sense and technical, the kinds of concepts, propositions, principles, and generalizations one uses in one's thinking, and finally a root metaphor, a world view, or metatheoretical perspective that is consonant with the other levels of analysis and synthesis. We believe this perspective to be the most fundamental intellectual achievement of this part of our research. Its test will be the degree to which it can subsume the substantive findings from the other parts of our study and several additional perspectives from the literature of social science and education.

For now, we have used Goldhammer as a means of articulating our perspective, essentially at the level of assumption behind the kinds of data we gathered and the kinds of substantive ideas and theory we will pose shortly. This is basically our conception of metatheory. To be more specific, the first six items on our list indicate we are making statements consonant with Pepper's (1942) contextualist world view and with Burke's (1945) dramaturgical model. Substantively we feel it puts us into the psychology of personal constructs of George Kelly (1955) and the dramaturgical sociology of Goffman (1959) as these have been brought together by Sarbin (1977) with the "enplotment metaphor". In education, symbolic interactionists such as Delamont (1976) and Hargreaves (1975) have a similar view.

Items 4, 5, and 6 suggest an historical framework and a systemic framework. In an early report on our findings, we used the term "longitudinal nested systems model" to capture our meaning. For clarity, we would note that the system's idea, for us, is not the

closed, convergent, mechanical model of the operations analysts but rather the open, divergent, holistic model suggested by the constructionists and the aestheticians and artists. These differences stretch into every aspect of learning, teaching, and schooling.

Item 7, innovation and change, is partly our attempt to deal with the initial substantive problem at hand, but also to capture planned or intentional action on the one hand and the larger category of unplanned alterations as well. In our view, these two concepts have been separated and kept too far apart.

Item 8 attempts to dissolve one of the major dichotomies of the logical positivists (Joergensen, 1951), the split between the operational, data language, and the theoretical, conceptual language in favor of a more configurational, concatenated, or patterned language and account of events. This implies a shift in the concept of explanation from a covering law model (deductive nomothetic or inductive statistical) to a pattern model (Hempel, 1965; Kaplan, 1964; Diesing, 1971).

Item 9 tries to focus on two points. The first is an attempt to be wary of judging individuals and groups, particularly from an earlier time and place, against a latter day set of standards, ideals, or ideology. The second aspect of Item 9 blends with 10-13 and attempts to take a position on the is/ought dichotomy of Hume, its extension by the positivists of the early 20th Century, and its part within the dominant ideology of American educational researchers (e.g. Campbell and Stanley, 1963) if not more world wide social science.

Item 13 is fundamentally an acceptance of R. M. Hare's (1952) decision of principle. When everything has been said, which is the extended account of all the items, then one has to decide how one wants to live--and then do it. That's our understanding of his decision of principle. Finally, Item 14 individualizes and personalizes that.

As one traces out one's assumptions and the roots of the assumptions, an awesome intellectual agenda is created. While this came to focus as we read Goldhammer's book, it obviously had been brewing for some time. Equally obviously, it is a long way from being finished. For us, though it sketches the level and kind of issues we believe are at stake at this point in our work and in our interpretation of where educational thought should be moving. More specifically, in the context of historical and ethnographic methods, the synthesis within and between groups of historians and anthropologists needs to involve items and issues of this sort.

4. Summary and Conclusions

These remarks began with the telling of a story or two. Beyond setting the general stage, my intention was to convey the potency of narrative accounts in explaining the origin of a set of ideas. Although they are history with a small "h", as Hexter used the expression, they do say some things about communities of researchers, our small niche of qualitative case study inquiry, and the sociology of educational research.

Secondly, my intent was to share with you a number of items that arose as I stumbled about in the "Board Minutes" of what has become one of the most interesting research projects in which I have ever been

involved. Mostly it's an account of attempts to raise some of intellectual and emotional reactions to the experience. Most grew out of interpretive asides, noted along the way.

Thirdly, as I detached myself from the details of Kensington Revisited, I was struck by the diversity among anthropological ethnographers and among historians. One might say that the within group variance was greater than the between group variance. In an earlier essay on ethnography, the reading I had done on Malinowski, Whyte, and Geertz reaffirmed what Paul Pohland and I had meant when we commented that there was no such thing as "standard participant observation methods." My more recent tour through the historians Gottschalk, Hexter, Stone, and White (not to mention such philosophers of history as Scriven, Dray, and Gardiner) left me with much the same feelings.

However, it seemed to me, and I raised a number of illustrations, that the kind of thinking processes we engaged in in our educational ethnographic work had a number of counterparts in the activities and thinking of historians who examine and work with documents. By starting with the hunch, the hypothesis, or the interrogative question, that history is nothing but participant observation with data fragments, I found the historians clarifying, extending, and rationalizing some of the more puzzling ethnographic practices we seemed to be using at a common sense level. In this way, we came to know better what we were doing and what we might do. We gained confidence that we could defend the practices. In addition, I believe we have both broadened the scope of our activities and moved toward a more patterned synthesis of a position. Though it's not "the" system or "the one right way", to

borrow a phrase from one of our Kensington collegial subjects, it does represent an approach others can look at, discuss, and criticize on the way to developing their own positions.

In this same, more detached mood, I raised two broad ideas which seem to further a more general rapprochement between ethnography and history. When, in the Kensington Revisited project, we begin to talk about "a longitudinal nested systems model of educational innovation and change" we thought we were on to a way of organizing our data to give a contextual or concatenated explanatory vehicle. We believe it does link historical and contemporaneous events into a potent image or framework. The more we puzzled with it, talked about, argued with ourselves and our colleagues, the more pleased we were. Finally, when we saw the similarities to the earlier classroom models we had developed concerning the micro processes of Geoffrey's classroom, we thought that we could subsume some of the most important events in schooling within similar frameworks. Whether that's really so, we shall see.

Mixed in with these activities is what my colleague Arthur Wirth and I have been calling a "search for a paradigm." For me, the most recent aspect of that search has been the perplexities which arose in reading Goldhammer's book, The School Board. Our Kensington Revisited account seemed so different. In detailing those differences, our thoughts skipped quickly through a number of puzzling epistemological and ethical issues, ending up with a focus on R. N. Hare's "decision of principle." Whether we have put as much order and pattern here as I think and hope and whether it is as important as I think and hope, I don't know. For the moment, it seems a rationale helpful to talk about, if not explain, our efforts in a kind of inquiry which seems to put us

on to interesting problems, which seems to give us ways of coping with those problems, and which seems, interestingly, to blend what a number of people call historical and ethnographic methods.

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4.3 Educational Innovation and Life History...

Educational Innovation: A Life History Research Perspective^{1,2}

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1. AN INTRODUCTORY ILLUSTRATION

Coming to a decision on how to introduce our remarks on Educational Innovation: A Life History Perspective has been difficult. In a conference group such as this, some of you we have known for a number of years and some not at all. Some of you have been aware and some of you not of our trials and tribulations in doing and now, finally finishing our Kensington Revisited project in which we have carried out a fifteen year follow-up of the Kensington School and its original faculty. These audience differences helped shape the essay. Recently, in doing a final pass through our teacher interview protocols, we came upon a section of an interview which seemed to illustrate a number of methodological and substantive items we want to raise. We offer it as "an introductory illustration."

When one rereads interview protocols taken a year or two ago, or ones carried out by a colleague, the interpretive if not hermeneutic process, takes on an interesting complexion. Conversations between the two researchers have occurred, joint interviews have occurred, the analysis of earlier interviews and the development of themes is underway. But here is one of us one day in July a couple of years ago:

OBS: The first day of July, is that right? And it's about 10:30 and we've just come back from an hour--hour and a half tour of Claire's school--Parkdale--and it's a K through 5. And a beautiful school. I guess I'd just like to talk about that a little bit. You walk in to the entrance and there's a fish pond with live fish and some birds, I don't know what they are--cockatoos or something?

Claire: I don't know either. Brightly colored, noisy, active birds.

OBS: Yes, they're really neat. And the entranceway is just very unschool like. There's a nice warm feel as you go in and a wide open space, just as open really as I think Kensington was as far as physical structure, isn't it?

Claire: I would agree with that.

OBS: This is where you--one other thing, I was just walking through and saw about 20 TRS 80 computers that would warm Steven Spanman's heart. Apparently there's a strong support from the Superintendent here for computers and particularly Radio Shack. We saw the area--are you working in that first and second grade--Pod is what you call it?

Claire: Pod is their name for it, right. We're in the first and second grade area which is a wide open wing of the school. And it's separated from the office area and it's separated from the other areas by a wall. But it's a very wide open area that would accomodate about--up to ten class groupings with some central facilities for lecture group meetings. An area which they designated Title I spot, kind of between a couple of bookcases where they have set up an area for us to work. But it's very much a feeling of openness. Everything is low. There are very few tall cabinets.

OBS: Carpeting all over with little carpet covered concrete steps going down, I don't know what you call that....

Claire: That's called the "pit."

OBS: Also much like Kensington with the same feeling, the group story telling....

Claire: Well yes, for children to go and wait for a group, to go outside, or go to play a game when they're finished with other work. It's a very multi-purpose kind of an area. There is carpeting throughout which is very nice for bringing small groups together. They can sit on the floor very conveniently. It's easier to keep clean.

OBS: I was impressed with the physical--not only the beauty of the place but the amount of equipment. Your district, you know the examples again--the TRS 80s of course are district-wide and are being brought in, but the physical education equipment--even that, which is there permanently, is pretty impressive. As I mentioned to you, the Xerox machine in which all of the reproducible, assembled materials that you hand out to students are actually Xeroxed, not dittos but....

Claire: We ditto virtually nothing.

OBS: Just because it's not as legible and it's kind of the purple...?

Claire: The purple. It's much easier to make up a Master, type it, write it, or copy it out of a book. The cut and paste things--you can cut pictures out, you can cut your material out, you can arrange it the way you want to, and then it can be Xeroxed.

OBS: Yeah, I'm just glad to see that is available. So often that's considered too expensive to do.

Claire: Evidently it's been cost effective for them because I'm sure you don't use any more paper and it's pretty much the same quality of paper.

OBS: And it clearly is more legible. Five to ten percent of those dittos wind up being smeared....

Claire: Oh, absolutely. And it's much easier to type up something, even if it's a teacher-made work sheet, make the corrections that you need to make--a white out--if you need to retype something--instead of trying to smudge it out or scrape it off the ditto--well, it's just a much neater, more professional looking job.

OBS: I wanted to pick up on a few things, Claire that you said in the Pod where you do your work. About how well do you seem to be integrated into the faculty and I've asked the question whether some of the classroom teachers resented the Specialist position you've got, to work one on one, while they were doing a really tough job of teaching a group. You felt very good about how you've been accepted.

Claire: Right. They see us very much as a professional working partner. They're free to bring over that day's work that a child needs reinforcement on. They're free to stop by and praise the child who we are working with, and very often they do. They'll come and say, "You should see the good job that Tim did today, what you were working on yesterday really helped because he was doing a terrific job." So they're very conscious of coming over and giving some positive fuzzies to the kids, as well as being able to get the help for them on the spot. We can provide reinforcement in their problem solving, whereas the teacher cannot always take that time out of a whole class to the extent that the children will work in, will need the help.

OBS: I think you were saying also since you're right in the proximity, instead of working independently for a few days,

you can immediately just walk 12 feet over and ask somebody. You'd ask the teacher specifically about the problem the child may have.

Claire: And while we don't make a practice of interrupting them in a skills group, if we really have to, they're very receptive to us. We can stop them in what they're doing and just ask a question.

OBS: And the free flow of kids you mentioned does not give them any kind of sense of being isolated or withdrawn?

Claire: Doesn't seem to be, at least with the first and second graders. We're part of their world and many of them stop by and say, "How do I get a Special Teacher?", "How do I get to work with you?", "What do I have to do?" And they very much would like that opportunity and there are so many children coming and going for different kinds of programs. They're going to Speech and they're going to Extra Reading with what--two or three different programs? These children are receiving help beyond the classrooms. It's a stigma not to get help.

OBS: Does this continue on all the way up to grade five? This special kind of Title I?

Claire: This program is Title I and that does not continue past third grade. We don't have enough funds to pick up that many children.

As we reread this, the meaning seemed applicable to a half dozen themes and issues:

1. If we ever needed evidence on the proactive nature of individual teachers determining where they will be--looking, choosing, trying out, deciding to stay, here it is. Claire, fifteen years later is a half-time Title I teacher in an open plan school with mixed kinds of cooperative teaching.
2. Claire is just one of several at the elementary level (principals and teachers) who have found Kensington-type spots. Several at the college and university level are doing things similar to Kensington fifteen years ago.
3. This Kensington Educational Innovation was not, is not, some fly-by-night fad for most of these original teachers-- including Claire.

4. Nor are the innovations "gone" from the American scene.
5. The ideas permeate multiple aspects of her beliefs, action, life style.
6. The technological solutions continue a pace--Xerox, computers, physical education equipment.
7. Social structural solutions exist, parttime specialists, professional working partners, in cooperative teaching relations.
8. The children's adaptability to varying instructional formats and styles seems apparent.

Methodologically, the protocol indicates issues in combining our lengthy interviews and on site observations, in combining the perspectives and styles of two interviewers, and in synthesizing the varied but interrelated timing of research activities: proposal writing, data collection and analysis, and writing of final reports.

2. PROCEDURES: BRIEFLY CONSIDERED

2.1 Restating the Problem

Initially we had phrased our problem around careers and life experiences subsequent to the first year of Kensington. From the first interview, partly out of our unstructured approach, partly out of seeing old friends from years ago, and partly from a never ending and unbounded kind of curiosity we talked of many more events. The early lives of our subjects/colleagues/friends flowed easily and naturally. We were into life histories immediately and the phrasing of our problem shaded off into broader and deeper channels. Briefly put, we undertook the development of life histories of participants based on two to seven hour taped interviews sometimes conducted by one of us, sometimes by the other but usually conducted by both authors present in the interview. Further, we had career resumes from some of the individuals who had recently changed jobs. Finally, a few of our faculty members had written and published on topics relevant to Kensington; we tried to gather and read a good bit of this.

2.2 The Life History Vehicle

We share the concerns and trepidations expressed by Robert White (1952) in his classic study, Lives in Progress regarding the frail vessels in which research data must be gathered. Each method, regardless of its position on the quantitative or qualitative, the

structured or unstructured, the controlled or naturalistic continua has its peculiar assets and liabilities. White expressed his concern as follows:

...it will be clear that the study of another person is a difficult undertaking which cannot be handled in a cut-and-dried fashion. Perhaps the very first thing to consider is the other person's motivation for taking part in such a study. Unless his interest is enlisted to a rather unusual extent he is not likely to be disposed toward wholehearted participation and candid self-disclosure. Even when cooperation is perfect, a further difficulty arises from the very nature of the material. No interviews or available tests, no existing methods of observation, can possibly be considered complete or definitive. Furthermore, all methods yield information of a sort that leaves much to be judged and interpreted by the examiners. In this way the frailties of the examiners enter the study and constitute a liability in reaching valid conclusions. It clearly behooves us to reflect a little on what is involved in trying to understand other people.

(White, 1952, pp. 92-93)

Balanced against those cautions is the compelling power and richness of life histories for our purposes. Our aim was similar to that of Goodson as he described the method and rationale of his recent study:

The genesis of the study reported in Curriculum Conflict 1895-1975 was to trace the reasons behind the promotion of a new school subject: Environmental Studies. The research method employed was to begin by collecting the life histories of the major participants in the promotion of this new subject. The patterns of decision, the changes of direction and the stated rationales given by these promoters of the subject were echoed in the evolutionary profile of the subject which was later constructed. *In a real sense the life histories of these key personnel constituted the life history of the subject in question.* (Our Italics) (Goodson, 1980, p. 17)

In addition to the fascination and richness of life histories for their own sake, we were drawn to them as ideal for our purpose of exploratory processes which occurred between the time of the original

study and the current life space of our teachers and staff. In effect, we had entered the lives of twenty-one professional educators at a point in time when they were beginning an exciting venture of developing Kensington and then we returned fifteen years later to again collect data in an intensive one or two day interview. The two studies were similar to selecting two frames from a full length movie for inspection and making comparisons and contrasts between them. Of utmost importance for our study was the inference of processes prior to and since the original Kensington. For example, we were puzzled by the origins of educational ideology which culminated in the decision to respond to the exciting and frustrating venture of building a new and different school. What varied routes did our educators take as they were being socialized on the farm and in the hamlets of America? Similarly, what had occurred during the fifteen year interim to influence the maintenance and/or modification of these same educational perspectives? Becker (1966) argued persuasively for the life history in the following words:

We can, for instance, give people a questionnaire at two periods in their life and infer an underlying process of change from the differences in their answers. But our interpretation has significance only if our imagery of the underlying process is accurate. And this accuracy of imagery--this congruence of theoretically posited process with what we could observe if we took the necessary time and trouble--can be partially achieved by the use of life history documents. For the life history, if it is well done, will give us the details of that process whose character we would otherwise only be able to speculate about, the process to which our data must ultimately be referred if they are to have theoretical and not just an operational and predictive significance. (p. XIV)

The emphasis in our research on an organic wholeness is deliberate and particularly useful when examining illusive threads of earlier

socialization processes which have all but disappeared from consciousness. Subjects would attempt to recall reasons for educational beliefs and would frequently digress into a story about their first day of school or life on the farm or about a pet they raised and light would be shed, albeit obliquely, on the topic at hand. Goodson shared our concern in the following words:

In understanding something so intensely personal as teaching it is critical that we know about the person the teacher is: our paucity of knowledge in this area is a manifest indictment of the range of our sociological imagination. The life historian pursues the job from his own perspective, a perspective which emphasizes the value of the person's "own story." By tracing this person's life over time it becomes possible to view the changes and underlying forces which influence that person at work, to estimate the part which teaching plays within the overall life of the teacher.

(1980, p. 13)

We would particularly stress the use of life history sketches for unraveling threads as intricate as motive structures, personal value and belief systems. For instance, to ask participants to state reasons for undertaking the original task of building Kensington after fifteen years have passed is to invite superficial and self-serving answers. The answers themselves are current word molds but the original castings may have long since submerged from sight or recall. However, those current molds are more likely to be interpretable within a life history which dredges up childhood dreams and disappointments. A participant may recall joining the faculty because at that point in her life she was somewhat bored and sought a new challenge while playing down any latent zeal to foster social reform. This same participant might weave a fascinating story of a childhood spent righting wrongs, mending birds!

wings and serving as a resident story teller for young children which suggests a more active motive structure other than alleviation of boredom.

Our commitment to the use of life history material is clearly strong and enthusiastic and not born out of a lack of a "better" method for data collection. It grew out of our initial yet evolving conception of our problem and out of a variety of related methods we had used before.

2.3. Finding the People

In our original proposal we rather blithely described our task to "...locate, observe, and interview the two dozen key administrators and teachers who originated the school." By rather circuitous routes this search mission was accomplished and we smiled as we recalled Skeels' (1966, p. 28) advice concerning "flexibility, ingenuity, and tenacity" as qualities to assist in the search. Upon reflection those are characteristics deemed useful and we would underscore tenacity as perhaps the greatest virtue.

2.31 The Search

Our search could be described as a series of post holes drilled into our social and professional networks with the pursuit of leads from each drilling until we reached a dead end. We then repeated the drilling in a new location. The first and easiest step was to locate the several people who had resided in the immediate vicinity and could be located through the telephone directory. This step was easily accomplished, but our hopes for an intricate network of Christmas card

lists or other forms of address links were quickly dashed. We found, rather surprisingly, that very few links existed among the participants. In this initial foray, two couples and two individuals have remained in close contact with one another but beyond these pairs the routes from Kensington were varied and disparate.

In addition to these individuals still in the area, another group of participants was known to us as a result of professional contacts since Kensington and hence, easily reached. This group, all male, included the former principal, superintendent, and curriculum director who have maintained professional association membership. In general, their whereabouts were known to the researchers even though few contacts had been maintained during the fifteen year interval. One of the teachers was located through a chance event growing out of a national conference. A former participant of Kensington noted the Washington University label on a name tag of a colleague of the senior author and inquired about him. This led to an exchange of letters and his eventual participation in the study. Again, rather surprisingly, there was practically no interchange between and among members of this group during the fifteen year period.

Our next venture led us to a graduate school which had been known to be the next stop for two of our participants. One individual was immediately located but the other alluded us because of name changes. We were struck by the impact of name changes upon professional identities. Beyond making life difficult for researchers the larger issue of women "disappearing" due to marriage and the distinct possibility of being under represented in research pools of this nature

was a challenge which we chose to meet head on. We were determined to find everyone in spite of this difficulty and we eventually found all but one male teacher from the original group.

We attempted to trace individuals forward in time from their last known address after leaving Kensington but apparently too many moves had been made by our highly mobile staff to achieve results by this method. Failing in our efforts to move forward in time we went back to original records and phoned parents of participants who were listed in the cumulative folders stored in a back room of the Milford School District Central Office. This ploy yielded several current addresses and phone numbers and in addition located the one individual who had current addresses of five participants as a result of Christmas card exchanges. The "unlocking" of this group then provided us with enough new post holes to complete our search.

Two rather isolated efforts are worthy of mention in that they might be overlooked. One individual was particularly difficult to locate until a call was made to the church in the individual's home town which had been listed in the original records. Even though no living relatives of the individual were still in the area we were pleasantly surprised to learn that the individual in question had married, returned home, and was an active member of the church. Secretaries again demonstrated their value as repositories of information. A second effort involved pursuing other listings in a small town with the same last name as the participant on the chance that an uncle or cousin might assist us in our search and this yielded our "missing" person.

Ultimately we reached all but two: one individual, a young woman, had died recently. The other individual's whereabouts remain unknown.

2.32 Involvement of Participants

After locating a particular individual a telephone contact was established and following a general exchange of information regarding current whereabouts, family and job issues, the purpose of the study was explained and individuals were asked if a proposal could be sent and a second call would follow to determine the individual's willingness to participate in a taped two to seven hour interview on a broad range of issues related to Kensington as well as experiences prior to and following Kensington. With very few exceptions the responses of the participants were overwhelmingly positive. Individuals were often eager to re-establish contact and discuss the "good old days" as several phrased the experience.

We stress the eagerness of the individuals' participation because of the subtle interplay between our methodology and the purposes of our study. Robert White was faced with a similar problem in Lives in Progress and the following comment seemed to say exactly what we had in mind:

It is sometimes felt that the interview method puts the examiner at the mercy of whatever fictions the subject chooses to set forth. When we ask someone to tell us what he considers to be the characteristic and essential features of his life, we certainly give him an opening to regale us with falsified pictures, selected events, and highly colored interpretations. Even when he intends to tell nothing but the truth, we cannot expect him to cancel his unwitting defenses or set aside his cherished illusions. Under favorable circumstances, however, this very real defect in the interview method can be greatly diminished. Much depends on the subject's motives and the relationship he establishes with the

examining staff. Some people do not really like to have their personalities studied. They feel defensive and would prefer to study the examiners while keeping secret their own true qualities. Others participate willingly so long as they can fathom the purpose of the procedures but become resentful if they suspect the examiners of trying to learn something which they themselves do not know. The subjects described in this book were relatively free from these forms of resistance. The process of being studied was congenial to them, satisfying important needs and thus evoking their fullest cooperation. The pattern of favorable motives was quite different in each case, as we shall see in later chapters, but it was always such as to dispose the subjects toward judicious candor in discussing themselves. (White, 1952, pp. 93-94)

2.33 Sequencing the Participants

While many times the selection of people to interview at a particular time was simply based on expedience or geographical proximity, several factors guided our overall strategy. First, we began with people who could give us the greatest scope and coverage of issues which might be pursued in greater depth with later individuals. We sought people at each teaching level in the early stage of interviewing to assure coverage. Second, we chose to interview individuals in key roles, (e.g., Principal and Superintendent) during the middle phase to allow us to probe issues raised by early interviews but also to enable us additional interviews to follow leads provided by our key participants. We were struck by the criticality of key individuals and realized again our indebtedness to the participants for their willing involvement in the study. While each person added greatly we were made aware that without the principal's willingness to share his insights and observations we would have had a difficult time completing the study at all and even if we would have completed it there would have been huge holes left by key individuals' absences.

2.4 Interviewing

2.4.1 Dual Interviewing

Over half of our interviews were conducted with the participant and both researchers present. Initially we selected this technique because we both wanted to be present for the early interviews as we were shaping our guiding questions and didn't want to miss the early content emerging from the interviews. We fully intended to then split the remaining individuals and conduct the interviews with just one of us present. As frequently occurs, serendipity smiled and we saw definite advantages in our approach. First, the role of interviewer and observer can be interchanged allowing one researcher to "back off," pay attention to more subtle cues indicating tension, threat or other mechanisms, and then choosing to pursue those leads or redirect if that is more appropriate. Second, the observing one of the pair can chart out a line of questioning more leisurely and pursue that route to give the other interviewer a chance to relax and "reload." Third, the observer can go back over notes and pick up leads for further elaboration which is more difficult to do when in the actual line of questioning. Fourth, different interviewers develop lines of questioning which are highly efficient and each can develop a speciality with the "two-platoon" system of interviewing. Fifth, interviewers can cover for one another's mistakes very effectively. At times an interviewer phrases a question poorly and the other edits or elaborates the question. Again, there are times when one interviewer is simply having a bad day and the other carries a greater share of the load. The obvious necessity for two people to be attuned to one another's strengths and weaknesses is a

prerequisite for this technique. Sixth, the difference in personality match-ups allows a greater rapport with some participants than others. Sensing this allows the better match-ups to be utilized to a greater extent.

The obvious advantages of this method carry potential liabilities as well. Perhaps the most important caution is the need to be sensitive to the participant's response. The dual interview could be a very demanding and exhausting experience both because of the fresh supply of questions but also because the contrasting styles of the interviewers could become tension producing. The ethical responsibilities of any interviewer are doubly important when the additional pressure of a second interviewer is added. Also, if one is not careful, the two interviewers can spend time talking to one another and dominate the discussion unnecessarily.

In summary, we felt the interviews went very well. As we have indicated we were pleased to see the teachers; they, in turn seemed pleased to see us. In our perception the discussions were open, frank, and searching. At one point or another, most of the faculty made some comment about the research procedures. In one of the interviews, this interchange occurred as we were probing the basis of earlier decision making leading up to becoming a teacher at Kensington. It captures the essence of potency of the dual interviewing:

Teacher: I don't know, maybe--maybe I would have grown up enough in that time, I don't know, I don't know--it's interesting--we're talking about things that I haven't even told my therapist--(laughter)--the two of you....

LMS: When we play ping pong--it's a little bit unfair....

Teacher: No, no--you're asking the right--no, I'm not complaining.

PFK: On any issue, any time that we probe or raise a question you'd rather not deal with....

Teacher: No, what I'm saying is that here I've been paying this man all this money and he's not asked the right questions and a lot of your ping ponging is asking questions that are, you know, bringing out....

LMS: Germane to a lot of things in your life?

Teacher: Thoughts and things, right--doesn't have a whole lot to do with Kensington, but....

2.42 Interview Patterns

While variations existed, the usual interview lasted from three to six hours and was done in two or three sessions. Typically we began in the afternoon, taped for two or three hours, broke for dinner, returned for an hour or so after dinner and then returned for a couple of hour wrap up the following morning. Usually the dinner hour was spent with the spouse included and an attempt was made to provide participants with information about Kensington today and the happenings of other participants. Occasionally, the spouse joined in the taping at the beginning or end of the session but unless they were integrally involved in Kensington they played a very low key role in the interviews. But they did broaden the life context of the discussion and indirectly validated and emphasized or de-emphasized particular items in the stories.

2.43 Content of Interview

A gradual outline for the interviews emerged from our first sequence. We began with a simple request to either tell us about their life before Kensington or to chronicle the events of their life after Kensington. Usually the latter route was chosen and the first taping

was spent tracing the personal and/or professional odyssey of each person. Having brought us up to date, the usual ploy was to ask about how the individual became involved in Kensington and to then trace backward in time, their early home socialization and school experience.

While references and comments about Kensington would occur at any time the final session would usually deal explicitly with perceptions of the experience then as well as post-hoc reflections. We never used an interview guide or a set line of questions. We preferred the free flow of discussions following the broad outline described above.

2.5 Summary Observations and Interpretations

After the interview the researchers taped lengthy impressions of people and events. We often had hours driving back to motels and airports and almost always filled those hours with taped comments. We particularly worked hard to record our first impressions after the evening's interviews were completed. The comments were wide ranging and often highly speculative comments which yielded most of the themes which eventually emerged from the study. Typically, one researcher would venture an opinion, observation or interpretation, document it with one or more pieces of data from the recent interview and then yield the microphone to the other researcher who would usually provide additional documentation if in agreement or counter evidence if he disagreed and then he would offer opinion, observations or interpretations of his own. Thus we wound down from the excitement of our interviews.

In the free flowing exchange which characterized Summary Observations and Interpretations, the exchange of bias, opinion, and

evaluative statements was inevitable. Again, Robert White (1952) was forced to deal with a similar issue and we felt compatible with his reasoning as he came to grips with the often highly emotional responses elicited by particular participants. In his study the strong and contrasting feelings regarding Hartley Hale needed to be handled and were described in the following passage:

The final point to consider with regard to method is the judging and interpreting activity performed by the examiners. It is impossible to study another person without making evaluations, and it is hard to keep these evaluations from being seriously distorted by one's personal reactions to the subject. Hartley Hale, for example, is a man who evokes strong feelings in the people who become acquainted with him. These feelings in turn call forth definite preferences as to what the case material should reveal. Some people are impressed by his success and admire his rapid climb on the ladder of professional status. Some are particularly taken by his vigorous self-confidence and capacity for independent and masterful action. Some tend to emphasize his service and self dedication in the field of medicine. For people thus disposed it is natural to hope that the reconstructed story will be one of triumphs over adversity, a living proof that circumstances can be conquered by will power.

Other people react with envy to a life that has achieved the outward marks of success. They prefer to feel that Hale had an easy time of it. They contrast him with people whose financial difficulties were more acute and who faced other special obstacles such as ethnic prejudice. They experience a certain resentment that he constantly got by in school without working hard or even behaving well, that he was befriended by instructors, and that he took such great pains to win the approval of his superiors. People thus disposed feel almost gleeful when the material reveals anxieties, irrationalities, and other evidence of imperfect integration. They favor the idea that he is just a neurotic young man who by lucky chance has found a socially acceptable way of holding his neurosis precariously in check.

Others dislike him for his attitude toward clumsy subordinates and toward his long-suffering wife, feeling that he deserves the comeuppance of an unhappy future. Finally, his political views and values easily offend those who do not share them. Resentment may be felt over his opposition to socialized medicine, his Republicanism, his individualistic outlook, and his lack of interest in organized religion. In short, it is

easy on the one hand to give him an admiration too uncritical, on the other hand to yearn angrily for the chance to teach him a thing or two.

The study of personality is one of the most difficult branches of knowledge in which to achieve a judicious outlook, free from invasion by personal preferences and personal feelings about the subject matter. Even when the more obvious kinds of prejudiced thinking are overcome, there is still the danger of projecting one's unconscious problems and unwitting attitudes into judgment.

There are only two ways in which this difficulty can be reduced to relatively harmless proportions. One of these is to neutralize the distortions by having several different workers collaborate in making the interpretations. Especially if these workers are of somewhat different backgrounds and training, they can, to a considerable extent, cancel each other's personal rigidities of judgment. The second way is that of progressively teaching the examiners to overcome their rigidities and to achieve greater judiciousness through increased familiarity with their own personalities. Such education occurs quite naturally in the course of team studies of personality, as in fact it does for many people simply through the experiences of everyday life.

(White, 1952; pp. 99-101)

On other occasions we have attempted to speak to some of the same issues. (Smith and Pohland, 1974, 1975; Smith, 1979)

2.6 Reporting Results: Options and Dilemmas

One of the current debates within the life history movement lies in the relative emphasis on descriptive versus theoretical formulations in the reporting of results. It is not a pseudo problem. Prestigious contemporary authorities take vigorous and oppositional stands on the issue. Almost to the point of caricature, Coles and Coles (1980) vilify the theorists:

We have not been trying to extract statements from American citizens in order to construct self-important theories. Our nation's cultural landscape--yes, from "sea to shining sea"--is already cluttered, if not badly contaminated, by a large

and constantly increasing mass of "findings": the "data" of various social science research projects. Everyone's "attitudes" about everything have been, continue to be, "surveyed." We have been declared a bundle of reflexes, organisms that respond to "drives," a tangle of hidden and not-so-hidden psychopathology. We have heard our poor called "culturally deprived" and "culturally disadvantaged," our ordinary working people described as "one-dimensional," as in possession of a "false consciousness," and dozens of other not-so-friendly labels, by critics who are more sparing with themselves and their own kind. The last thing we want to do is come up with a few more pushy, overwrought, wordy generalizations about America's people, and specifically, its women, who have been getting a good deal of attention lately--not always discerning or appropriate. (1980, pp. 1-2)

After a reference to one of their subjects, who quoted a grandmother's comments, "You'll find all kind among all kinds," the concern seems a fear of stereotypes and overly simplified generalizations, the Coles then comment:

No doubt there are valuable ways of pulling together life's variousness into compact, suggestive statements. The point is to move from the particular person to the broader arc of humanity without violating the kind of truth that daughter of a slave knew in her bones. One old-fashioned and still rather lively, penetrating, and illuminating way of doing so is through the medium of a life-history (not to be confused with a case-history). Biographers know that through a person's story they can shed light on the stories of others, too. And novelists know that even a person imagined can do likewise--make the "real" seem closer at hand and more sharply focused: a paradox, and one of many in a world full of small as well as large ambiguities, ironies, inconsistencies, incongruities. Any psychological theory, any sociological scheme of interpretation had better do justice to all that--to the complexity of human affairs--if it is to pass the muster of a knowing daughter of a slave, and of her granddaughter. Amid all the structuring of life into "periods," "stages" of human development, with psychosocial variables and sociocultural factors, there is room for plain biographical presentation, with a vivid moment or two--stories of humor, of regret and sadness, of aspirations retained or dashed, of fears banished or never let go.

We have tried to shape what we have seen and heard into a kind of story--a life presented, with all the subdued tension and

drama, the dignity, and inevitably the moments of fear and sadness, that characterize most lives. (1980, pp. 3-4)

So the argument is made for biography, for life history, for narrative.

But creeping in, is the intimation that maybe its the wrong kind of theory that is under attack. The concepts used provocatively, to contrast with "periods" and "stages," are:

...vivid moment...
humor...
regret and sadness...
aspirations retained or dashed...
fears banished or never let go...
subdued tension and drama...
dignity... (p.4)

When arranged this way, their account is almost poetic. When each item is taken on, one by one, as a social science concept, with antecedents and consequences, in the best positivistic style (Zetterberg, 1965) a very difficult agenda is created. What does a theoretical social scientist make of "regret and sadness," of "fears banished or never let go," of "subdued tension and drama."

In contrast, Rosabeth Moss Kanter (1977) talks about men and women of the corporation. The accounts of managers, secretaries, and wives come alive as she accents: structures of opportunities, broadening the distribution of power, and helping balance numbers of men and women. As we thought again of Coles subjects—Laura, Marie, Sue, Marie and Elaine, their stories took on additional meaning.

It's not only the psychologists and psychiatrists who fight this way over the study of individuals, but every other social science

discipline. History, the heartland of the particular, has its theoretical, and sometimes quantitative, types who worry about forces, factors, and dimensions (White, 1963). Educational researchers and evaluators, while dominated by the experimentalists and theorists, do have their responsive, story telling, narrative types (Ryan, 1981; Hamilton, et al, 1977). Anthropologists differ as well. Sociologists are among the most vehement and varied.

Our own approach, over the years has been to give close to equal time to both narrative and theorizing, sometimes a little more of one and sometimes the other. And sometimes trying to blend and integrate stories and ideas in different ways. One of the best resolutions has been what Hexter (1971) called "processive history," an alternative different from narrative history or analytical history. Processive history involves knowing outcomes and building meaning and drama into the account using what he calls macro and micro rhetorical principles. Stone (1981) speaks of "stories guided by a pregnant principle."

A final interpretive point is that there can be very dull, boring narrative accounts from which one learns nothing and wishes time had not been wasted. Equally often, theoretical accounts appear that are obvious rather than penetrating and whose generalizations do not teach one anything. Rather, the trick seems to be in being clever, to see and to communicate in an interesting and exciting manner, items that readers find important and worth learning. And that is very hard to do.

2.7 Conclusions

The life history material developed in this study was subjected to multiple reviewers at different points in the process. First, with two interviewers, analysts, and writers the likelihood of one person's bias influencing the outcome is substantially reduced. We questioned and probed regarding the data base for generalizations derived from the tapes. Second, data have arisen from other times, places and sources. The most dramatic illustration concerns the Superintendent. The Milford District records include school board minutes and community newsletters. Some District "fans" have collected newspaper accounts from other times and places. Third, at different points in their lives some of our participants lives have crossed. As each presented small--and occasionally large--items of this sort we could cross check stories. Finally, the materials used were shared with the participants prior to publication to gain their perspective on the fidelity of our accounts. While these checks do not eliminate the possibility of less adequate, if not erroneous, interpretations, at least the frequency of their occurrence has been substantially reduced.

3. SPECIAL ISSUES, KEY DECISIONS AND STRATEGIES IN LIFE HISTORY RESEARCH

Several major difficulties or problems occurred in doing this life history piece, Educational Innovators: Then and Now (Volume V), of our Kensington Revisited project. Working our way around, through, and over the difficulties demanded, we think, the best of our creativity. In hindsight, the decisions seem simple, straightforward, and hardly more than common sense. But at the time they were hazards, precipices, and chasms. Such is often the nature of practical problems and their resolution.

3.1 Evolution of the Problem and Project Magnitude: The Interactive Flow

When we first began, our problem was a relatively simple two piece affair. Kensington Revisited meant 1) returning to the school for a contemporary ethnography and 2) finding the original faculty and seeing what happened then. As we were doing these tasks, items of data appeared, for example the student population of the school had gone from 100% White to 60% Black, the prior principal that everyone talked about was Michael Edwards, not Eugene Shelby, the principal from the first year, a conservative superintendent, Ron George, had not only succeeded Stevan Spanman, who had built the school in 1964 but had been there fifteen years, and the Kensington School, as we had predicted, was traditional and very similar to the rest of the Milford School District. These were major "facts" to be understood, interpreted and explained.

A more extended example elaborates the point. One of the early interviews of the original Kensington faculty was with Irma Hall, now retired. Her roots went back 50 years in the community. As she talked about her career she raised a number of historical items about which we knew almost nothing at the time. The seeds of the redefinition of the problem and the expansion of the project were present in the protocol of that early taped interview. As she traced her career she mentioned the fact that the Marquette School was a separate elementary district from Milford when she started and that there was a Black school in the community:

OBS: What about other communities in Milford? Was the school in Milford the only Black school in that community?

Irma: Well, I don't now, because I didn't get out into that neighborhood very much. And I don't know.

OBS: But obviously one of the reasons I'm curious is that part of the story of the current school is this huge shift in the Black population. And I don't know enough about the history of Milford, but I want to learn about it in the sense of what we are talking about here. To see what other kinds of things existed, say, before 1954. And I never heard anybody talk about or haven't really inquired about Marquette as a separate district and the fact that there was a school for the White children, a school for the Black children. And there may be others there--just that most of the older communities--in Suburban County--have small, indigenous Black populations, from whatever time.

Irma: Yes, yes. I think there were very few children in the Black school. I wish I could think of its name.

OBS: That will be in some record someplace that we'll be able to find.

Irma: Yes, I'm sure they have it in the school records.

OBS: At that time did they have a high school also, and a junior high? Or where did the kids go from Marquette?

Irma: They went to Milford High.

OBS: Even before the...

Irma: Uh-huh

OBS: So in a sense there was already...

Irma: It was only an elementary school--eight grades.

OBS: And I presume all the neighborhoods like Carlton Heights, Marquette, and so on, when they consolidated, mostly those kids had already been going to the high school.

Irma: I think so. Now I think, again it was a tuition matter. They had to pay tuition for the kids to go to the high school.

OBS: And the local district would pay that?

Irma: Uh-huh

OBS: Yeah, that whole business--I don't really know much about the total reorganization of the county school districts. I don't know--I assume that was in the late 40's or early 50's some time. And I don't really know that history well.

Irma: I'm not too well acquainted with it either, but I know it happened.

OBS: But you were living through at least the beginnings of all of that.

Irma: Oh, yes.

We found the records. Initially they were the Milford School Bulletins. Later, and more importantly, we discovered the gold mine called the School Board Minutes. Solving the problem raised in the conversation with Mrs. Hall cost us two years.¹

¹The methodological account, *Ethnography and History in the Study of Schooling* (Smith, 1982) was presented at the St. Hilda's Conference in September, 1982. Volumes I, II, III of the report deal with Milford School District and community, the historical and contemporaneous context of the Kensington School.

To handle those events, the project expanded in scope, shifted its title to "Innovation and Change in American Education," came to have a six volume final report, and has taken much longer to complete, six and a half years since the original proposal was written.

At the practical level the project obviously "got out of hand." We fought for additional resources, got some but not enough. Smith's project administration left much to be desired. Prunty and Dwyer were finishing dissertations on other topics and eventually sought jobs in a very difficult market--eventually finding spots in San Francisco and Geelong, Australia. Kleine made a major career move from Wisconsin to Oklahoma. The Graduate Institute of Education at Washington University entered a turbulent three year period and eventually was altered to become a much smaller Department of Education. But conceptually, the report expanded to fit the twists and turns of the problem, the available data, and the open reception from the Milford School District. In that sense, we were well in control.

But, the long and short of it, the methodologically and substantively new, to us, strand of life histories based mostly on extended interviews suffered in terms of time and intellectual resources. It became the last piece to be finished. Research projects as social enterprises do have their own dynamics and they do live in an environment, which of late has been turbulent.

3.2 Aggregate, Group, or System?

One of the most difficult analytical problems for us in Educational Innovators, Volume V, lay in the fact that our previous field research

was always about an intact group or social system. Geoffrey's or Brock's classes (Smith and Geoffrey, 1968, Brock and Smith, 1980), a CAI project (Smith and Pohland, 1972), a school district science program (Smith, 1978), and so on. By intact group or system we mean simply people in communication, often face to face, and usually in a common physical setting. The doing of things together usually had elements of common purposes and goals, sometimes over a short defined time period, but often over much longer stretches of time. As observers, evaluators, or sometimes collaborators of an unusual sort we have entered the ongoing systems, participated in varied ways and tried to report on the experience (Smith, Dwyer, and Prunty, 1981).

Our Educational Innovators were a funny or unusual kind of a group. They had been together for a year, and a bit more in some instances, as dyads or subgroups. But for the most part there had been minimal contact over the last decade or decade and a half. Could we approach our interview protocols from these not-now-in-contact individuals in much the same way as we have our field notes in the past when they were based on an ongoing group or social system? Or did we in fact have an aggregate of people? But if it were an aggregate it was a funny one for it was neither a random nor representative sample from a known universe of some larger defined aggregate. Rather it was nearly all of the population of a group who had been together fifteen years ago. (In some ways all that was holding them together now was us, the researchers who were now back into their lives. This seems an important and provocative

point, and one shared with historians and biographers. Historians are the links connecting disparate events and people. Biographers connect events and people who have been "lost."

Flitting through all this was Katherine Porter's novel Ship of Fools, which we had started years ago and returned to more recently as a possible literary model, analogy, and metaphor for use in thinking about our problem, our data, our analytic forms, and our report. Her Narrenschiff seemed fertile and provocative in multiple ways, not the least of which was the fact that she spent half a lifetime working on it--as we seemed also to be doing with Kensington Revisited.

But the point we kept going back to, time after time, when we seemed lost or out of focus was the uniqueness of the group, individuals who had once lived and worked together. They were of a piece, they had been together for a year. By signing on together, so we argued in Anatomy (Smith and Keith, 1971) they identified themselves as innovators, true believers concerned with the possibilities of the new elementary education. In some fundamental way it seemed they could be treated as an entity. Such became a major metatheoretical and controlling hunch for the analysis.

When such a hunch or hypothesis becomes a metatheoretical assumption it guides and controls what one does. As we comment elsewhere we had difficulty in reporting on individual's qua individual's as opposed to collecting bits and pieces from several individuals to make a point. Our compromise if not integrative solution was to try to use a single individual to make the basic point of an

analytical theme. In effect to get out his or her story mainly in one place and to use summary tables and figures for one kind of generalization.

But the issue which came clear when we thought of the group as a group, and the big gamble, was that the individual interview protocols, and the item within those protocols could be treated as pieces of a larger conceptual whole or entity. Our themes of "educational reform as secular religion," "you do go home again," and "roots of educational ideology" while initially focused around one or another individual could be elaborated by reference to others from the group. As we built our models, e.g. "toward a theory of the secularizing of religious belief into educational ideology and reform," we could use an item from here melded with an item from there, and with an inference from somewhere else.

The intellectual, theoretical castles, or perhaps better, houses of cards, we built seemed more ephemeral than ones we had built on other occasions and in other volumes of this report. While much more hypothetical than the models we have built before, we believe we are still generating theory in the best sense of Glaser and Strauss' (1967) use of that term. Our defense seems to rest on answers to several questions. First, have we had some insights and built some syntheses which are both original and potent, that is, enable others to think about innovators and innovations in ways they have not before? In effect does it meet a pragmatic criterion? Second; do the syntheses hang together? Is there an internal coherence to the argument? Third, as we turn the materials back to our group, do they see themselves both

veridically on the one hand and with additional but believable new insights on the other hand? Subjective adequacy is Bruyn's (1967) label for this criterion. Fourth, can other researchers, be they qualitative case workers looking at other collectivities of educational innovators (eg. Goodson and Countesthorpe, 1982) test our ideas? Or can social scientists from a more quantitative, survey or experimental tradition find opportunities for corroboration, verification or falsification of parts of the schemes?

But the point we are really making is simple though arguable. Even though the data are from interviews of individuals now separate and apart from each other, we remain interested in the problems of the interdependence of innovations, education, and school organizations as well as individuals as individuals.

3.3 Holism, the Individual and Situational Analysis

Tucked into the discussion of the aggregate, group, or system was a comment about portraying the themes through an individual case. That was really a bigger issue, a problem which took a while to solve. A summary observation and interpretation note captures the flavor of the problem. Once into it a number of related items seemed to flow together--"themes in lives," "it's their story but we are trying to understand it," and "situational analysis."

3.31 The Initial Anecdote

Today, I awoke at 4 A.M., tossing, turning, and twisting with a number of problems and discontents bothering me. It was a different kind of restlessness than the usual apprehension, anxiety and guilt on the negative side or anticipation, hope

and excitement on the positive side. It took me an hour before I realized that the final struggle was on with Volume V of Kensington Revisited, Educational Innovators: Then and Now. The writhing-about took on a pleasant quality and forced a smile to my face. I recognized that I was ready to come to grips with the last of the big demons from the project. It wasn't as though we hadn't worked hard up to this point, for we had. We had interviews on all the people, we had done preliminary papers, we know many if not all the major themes, but somehow we weren't ready, or I wasn't ready, until this morning to write it.

(Summary Observations and Interpretations, 10/82)²

One major difficult issue was the formatting of the data, the interviews and observations of the 20 educators from Milford and Kensington. We didn't have a simple single case, i.e. a class, a school, a curriculum, or a district. Rather, we had 20 cases, that began in 20 different places, intersected for a year at the Kensington School in the Milford School District, and then diverged once again. Preliminary discussions (and papers) indicated a half dozen major themes. Somehow, a content analysis of the 20 cases around the six or so themes seemed to do an injustice to our individuals, to the general intent of our "holistic, contextualized theory generating case study thrust" of the last two decades. The resolution involved selecting and possibly throwing away fascinating data and stories, by using one individual's life history to carry the brunt of each of the themes. Alec Thurman, one of our teachers, seemed to be the prototype of "Educational Reform as Secular Religion." Methodologically and procedurally the hypothesis to be checked was whether we could make that work. What would be gained and lost? Could we tuck in the other individuals and the nuances and subtleties that their lives contributed

² Parenthetically, we would write that another semester of teaching and other activities intervened for each of us.

to the idea without distorting them, the data, and the theme itself. That seemed an appropriate if tentative resolution, the big idea that was struggling to get out, that brought a smile to Smith's face at 5 A.M. In retrospect, or from an outsider's perspective, that might seem like a long road to a very small house. But from the inside it was the best of creativity. When one is stuck, overdue on reports, and writhing around at 4 A.M. even little pearls should be cherished. Figure 1 suggests the context at the time of solution.

Insert Figure 1 about here

As that idea took hold we kept searching our records for an individual who might serve as prototype for each of our themes and subthemes. In working through our career analysis, Superintendent Spanman seemed to reflect upon a number of issues relating personality, organizations, and careers. As these issues tumbled out in our life history career vignette we found other items relevant to other themes. We elected to present them at this point to maintain an integrity and a holism about him and his perspective. In effect, the data spill out over other issues, a point we return to shortly in our discussion of situational analysis.

3.32 Themes in Lives

One of the surprising methodological and substantive outcomes seems a reflection of a human being's propensity as a meaning maker. The disposition seemed to be in the interviewee, the interviewers, and the relationship developed in the process of the interview.

1. Varied (false/initial) Starts

2. Division of Labor

3. No Clear Idea of the Overall Product

4. Finishing of Volumes I and II: History of Milford

5. Finishing History and Ethnography Paper--Oxford

6. The Oxford Conference and Conversations with Ivor Goodson

7. Mary Burke's 509, and deCharms comments on Murray and White Lives in Progress, and Beittel's Alternative for Art Education Research

8. Classes in Field Study Underway and Moving (Wrestling with Students' Problems)

9. Press from CEMREL, Toronto, and Riverside re: Presentations

Idea of Extended Case Reporting.

1. Book of Cases

2. Mostly One Case Per Theme

3. Begin with Educational Reform as Secular Religion Theme: Alec Thurman

4. Already Have Spanman and Career Diversity

Progress on Volume V

Figure 1: Antecedents of the Idea of Extended Case Reporting

The development of the insight grew out of the reading and analyzing of the protocols themselves. In particular, David Nichols' accounts of his religious rebirth and the puzzling that that created for us seemed to be resolved mostly as an account of and a distinction between the substance and structures of belief systems. Though he said a number of other provocative things, many of which appear scattered through the report, it was the surprise of that announcement that crystallized the interview and unknowingly at the time, much of the later analysis. Similarly Sue Norton's mentioning of "not belonging in education" became the focus of much of the discussion, the later analysis, and the account of her in the report.

Concurrently with the final analysis and writing of the report Smith has been involved with Professor Arthur G. Wirth and a dozen students in a seminar entitled Search for a Paradigm: Education, Images of Human Nature, and the Human Sciences. Although that is a separate enterprise, it cuts across the Kensington Revisited project in several ways. Part of the report has been under discussion in the seminar. Much of Smith's interests in the topic have come to focus on the assumptions underlying the Kensington Revisited effort. Arthur Wirth's personal thrust has been toward man as "homo poeta," the meaning maker. Among the numerous items we have been reading (Becker, 1968; Bernstein, 1978; Mills, 1959; Fay, 1975), Bernstein's search for an empirical, interpretative, critical theory has been a major source of stimulation. It seems to capture our move from a more positivistic to a more interpretive symbolic interactionist, if not critical theory position.

In sum, a number of influences have coalesced in our movement toward "themes in lives." In part, as always we seemed to do better than we knew when we interviewed, for the interviews run back one to three years from the date that these notes are being written indicating the current attempts at a rationale for what we had done. As always also, the product seems some blend of "what's out there" and our insights and labors in construing these materials. Figure 2 captures the particulars in schematic form.

Insert Figure 2 about here

3.33 "It's Their Story, But We're Trying To Understand It"

Once again we find our volume of the final report, Educational Innovators: Then and Now, filling up with excerpts from the data. This time the excerpts tend to be verbatim quotes from the interviewees. As the report grew longer and longer, we had some qualms that maybe we were just taking the easy way out, rather than reading, digesting, and presenting a more tightly-drawn essay. The phrase that began going through our heads was "it's their story" and shortly afterward "but we're trying to understand it." In effect we wanted to bring the reader as close to the actors' world as we could, and then, and the "and then" is important, try also to show the reader the sense we had made of the overall experience.

We believe this is consonant with the more interactional and contextualist view we have in the metatheory of research methodology. There is no pure--out there--in the positivist's sense, nor are we

Individual presentation
of self as having an order

Interviewers' attempt to
raise initial themes

The flow of the conversation:
surprises, contingent responses,
attempts to integrate

Reflective analysis of
protocols within and
between individuals

Writing the report

Concurrent interaction
with Professor A. S. Wirth
and the 611 seminar

A life as a
meaningful
theme

Clarification,
Project results
and Report

Figure 2: Life as a Meaningful Theme:
The Merging of Methods and Theory

fabricating a world totally in our imaginations. Rather we found, from the initial definition and statement of our problem to the revisions in the statement along the way, this interactive quality. Similarly the kinds of probes in the interview and the responses they elicited were always in the context of the special relationship we had over fifteen years and the prior involvement in what for all was a significant emotional experience. Finally as we read the taped protocols, and reacted to them once again with a distance of several months to over a year, we found ourselves wanting to let each individual speak for himself or herself. But we didn't want the reader to meet the staff totally alone or in isolation because we had been a part of the generation of the data. Consequently we kept framing the responses in the meanings that we had generated in those continuous iterative interactions we had had. Now the readers of our reports bring their own backgrounds to the data and interpretations. Some will bring a knowledge of Anatomy, and others will bring knowledge of their own experiences with innovative schools and educational reformers. At some point, assuming we have had something interesting and worthwhile to say, a new "our" view is created as the various readers join in.

If this is the way things are in our research then the problems of evaluation take on a different complexion. And if our instance is more of a prototype of the research process than most methodology texts indicate, then the research community has an interesting larger problem on its hands than it's been willing to acknowledge. In part our methodological essays and appendices have been an effort to keep chipping away at that kind of clarification.

3.34 Situational Analysis

While this accent on larger chunks of interview protocols and individual lives sometimes leaves the central conceptual point a little "ragged," it does prepare the reader for another very important point, what Van Velsen (1967) has called "extended case method and situational analysis." In his provocative, and, surprisingly, little cited essay he comments:

I have called this method of presenting and handling ethnographic data "situational analysis." By this method the ethnographer not only presents the reader with abstractions and inferences from his field material but also provides some of the material itself. This puts the reader in a better position to evaluate the ethnographer not only on the basis of the internal consistency of the argument, but also by comparing the ethnographic data with inferences drawn from them. Particularly when several or most of the actors in the author's case material appear again and again in different situations, the inclusion of such data should reduce the chance of cases becoming mere apt illustrations. (1967, p. 140)

Van Velsen makes several additional points which parallel positions we have taken at several places in our substantive reports. For him this includes: 1) Vignettes of individuals, their choices, reasons, actions, and reflections are an imperative; 2) The analysis moves from a synchronic to a diachronic form. In our terms one goes from structures to processes. We ended up with biographies of individuals and a "longitudinal nested systems" conception of Milford School as a District; 3) This stance permits analysis of conflict within and between individuals. For us this moves immediately to a more interactive and political type of theorizing to accompany the biographies and life histories; 4) The concrete and the idiosyncratic become part and parcel of the analysis. For us this begins to take on the flavor of

particularizing which has long been an important part not only of life histories but also historical inquiry and ethnography. That seems an important kind of integration; 5) The need is raised for accounts of "inner perspectives" to go with our more detached "outer" perspective. The best phrasing of this, in our view, is Geertz (1973, 1975) "experience near" and "experience distant" distinction;³ 6) In its farthest reaches we believe this sets us on a course toward practical theory, the blending of the is and the ought, what sometimes goes as scientific and normative theory. And that is a major charge (Hirst, 1973; Bernstein, 1978).

In effect, a troublesome analysis and writing problem, seemed resolved one restless night. The blending of ways to proceed with the report and major ideas, themes in lives, then was further rationalized and perhaps legitimated with appeals to multiple disciplines and approaches in history, anthropology and psychology.

3.4 Specific Techniques on the Search for Meaning

In any craft, such as gardening, fishing, or clockmaking, knowledge can be concrete as well as abstract. And oftentimes it is the concrete which is the more helpful. In our view, the doing of research is very much a craft. Concurrently with our abstract exploration of the "interpretive paradigm" (Fay, 1975; Bernstein, 1978; Geertz, 1975) we also noted some concrete ideas or suggestions which seemed to specify the tacit meanings in the day to day doing of our research and which extend the position we are raising.

³See the essay Ethnography (Smith, 1982) for an elaboration of Geertz's (1973) concept of "thick description" which is important for this discussion.

3.41 People Who Talk In Quotes: A Mixed Blessing

In an earlier study (Smith and Dwyer, 1980) one of our subjects told us, "You should be sure to interview X; he talks in quotes." That comment has remained with us. Our informant was himself an experienced interviewer and field worker. He knew the power of such an interviewee for vividness in the narrative and ideas for the analysis. A person who talks in quotes is someone who is fluent, who runs with open ended probes and who tells well interesting stories with a point to them. Our original Kensington faculty was full of such people. Here we want to speak of the mixed blessing such people create.

Our first and major point is our opening one--vivid stories with a point outweigh all the disadvantages. In a sense, the most specific, concrete, practical technique in the "general search for meaning" is to find subjects "who talk in quotes," individuals who have already done the job of observing, reflecting, conceptualizing, and narrating an important perspective in the experience.

Where time is short or one is overdue with a final report and one has fifty to a hundred single spaced pages of transcript per interview and one runs into an "individual who talks in quotes" then one has a problem of another order. Every page represents a new idea, a significant issue, a concrete specification that vividly makes a point. Continually, one runs to the Xerox machine to cut and paste one more item that's too good to be lost or omitted. The day or two one planned to spend with that interview becomes a week or two of concentrated hard work, not a casual run through to check, verify, or qualify a set of general points already well in hand which only need minor elaboration.

And the report grows, swells, becomes overwhelmingly long. It forces one to reconsider what it is that is most important to tell. What are the main themes that need exploration? And then the outlines collapse. And then the problem is reconstrued. And on and on.

A bigger problem is that one must be careful that the fundamental story, analysis, and interpretation is not overweighted and skewed by one or two such individuals. This seemed less a problem in Kensington Revisited because so many of the people talked in quotes.

Figure 3 presents our overall theoretical summary of the phenomenon of subjects who talk in quotes.

Insert Figure 3 about here

3.42 "What Do You Make Of This?"

Several years ago, in a paper on teacher effectiveness (Smith, 1972) we hypothesized about the relationships among "homey labels," the internalization of the central constructs of an area of study (in that instance, aesthetic education), and the teacher's ability to improvise. Recently, in talking about field work as we were trying to teach it, the observation was made that much of the red ink on students papers was one of those homey expressions, "What do you make of this?", or reasonable facsimiles of that question. Usually the student would have reported on an incident or event and left it there. The "What do you..." intervention seemed to imply several interrelated meanings. 1) Is there a more holistic context in which the item might fit? 2) Do the participants impute any special meaning into the event? 3) Do you as theorist see

1. Vivid story telling
Concrete episodes
Sense of dramatic structures
Each makes a point

2. Reflective intellectual style

3. Having a personal point
of view

4. Open to discussion about
the point of view

5. General intellectual ability

Talking in
Quotes

Over use of
particular subjects

Enriched description,
Analysis and interpretation

Writing the report
for the investigators

High
Quality
of
Report

Figure 3: On Talking in Quotes

any more abstract and general implications? 4) Do those abstractions fit in others' systems: a functionalist?, a behaviorist?, an interactional?, an interpretive?, or a critical perspective?, that is how would any of them see it? 5) Or would those others be categorized better as psychologist, sociologist, anthropologist, or educationist? 6) Finally, what does it mean to you, Mr. or Ms. Jones, participant observer and interviewer who is generating his or her own brand of theory?

To return to our initial illustration, "the homey label" or question reflects the important issues of the construal of theory in field work research. The alternative phrasing suggests the need for an ability to improvise in the analytic aspects of field work.

Volume V, Educational Innovators: Then and Now, was a very different set of data than any we had had before in our work. In the past the core of our data were direct observations of an on going social system. Interviews tended to be offhand conversations or occasional ancillary and supportive efforts. We had been in transition through the Alte Science Education Project (Smith, 1977) and the Federal Policy in Action project (Smith and Dwyer, 1980). But here we had to face the major difference, the core of the Volume V data were from interviews. But, as always and as we have indicated, there are disclaimers. We had known the people intimately fifteen years before, when they spent the year at Kensington (Smith and Keith, 1971). Second, a few of the people we had met professionally on and off through the years. Third, several of the individuals had published materials which we had seen or which they provided us when we interviewed. Fourth, over the years, some of

the people knew and worked with people we knew and we had bits and pieces of information about them, their activities and their lives. But basically, the data were the interviews.

Analytically, the interviews posed a problem. After a number of conversations, false starts, and partial successes, the generality of the homey expression "What do you make of this?" arose. In another context we had been working on field work issues and we commented, "one way to make ~~concrete~~ the move from a more positivistic perspective to an interpretive perspective is in the phrase we keep putting on student papers, "What do you make of this?" Later upon returning to the interviews, it seemed possible to see the interviewees commenting upon Kensington, their careers, their lives and that each paragraph, or sometimes sentence, could be questioned in similar fashion.

Now, as we read the interviews, we stop and ask. Items arise, a reach for theory is made. The items accumulate. We seem back in business, much as before.

3.43 Go for the Insights

The numerous "individual cases" making up the overall case of educational innovators, provoked all kinds of methodological problems in the analysis, as we have indicated at several points. One of our tactics, as we struggled, were blocked, and struggled again, came to focus in the label, "go for the insights," work them out one by one, and let the configurations and patterns come later.

In a sense this is merely an adaptation of our earlier commitment to the importance of "interpretive asides" in our field notes (Smith and

Geoffrey, 1968; Smith, 1974 and 1979). This time, the question became "What are the big and little things we learned from the interview with individual A, with individual B, and so forth through the list. From Volume V, Spanman's comments about the school administrators network and playing the game were vivid and powerful, David Nichols' born again religious experience raised the distinction between content and structure in belief systems, and Alec and Marcie Thurman's ability to actualize interdependent careers, all illustrate the methodological point.

As we got those issues described and under some kind of intellectual control dozens of other subpoints arose. Patterning them within the individual and across individuals became very important.

Further, the two of us as observers, interviewers, analysts and writers had only partially overlapping points of view, agenda and interests. The tugging back and forth to develop a single point of view altered, expanded and tempered items toward the final product.

Distances between St. Louis and Kenosha and later St. Louis and Norman, Oklahoma and the long time involvement several years on the project and 20 years of colleagueship both complicated and enhanced the effects. But the central point remained, what are the insights, sentence by sentence and paragraph by paragraph from the protocols and interview by interview through the twenty individuals.

3.44 Brainstorming

Recently in talking with a graduate student doing a field study (under the tutelage of a colleague at another institution) I found myself listening to an account of the need for rewriting some of his

materials before he had, in my terms, fully exploited his data for creative ideas, interpretations, and grounded theory. It seemed not only like the wrong time for rewriting and revisions but also that the "orderly thrust" lying behind the suggestion was different from the "disorderly mode" we have always used.

Then, one Sunday morning after a day's rest, and after beginning a final pass through one of the interviews which was yielding all kinds of ideas and qualifications related to our themes of "going home," "the natural history of belief systems," and "the volunteer resolution" of the marriage, family, career dilemmas of the younger women who had been on the Kensington faculty, it struck us that "brainstorming" was one way of couching the problem of the analysis of field notes and open ended interviews. We had never made this connection before.

The analytical process that we fell into years ago was simple: 1) read along; 2) stop after each item--sentence, paragraph, or longer more involved episode; 3) ask oneself what does it mean; 4) figure out a tentative label or heading; 5) write a paragraph or few pages quickly to exhaust the immediate meaning; 6) build a little sketch or tentative model of any larger conceptualization (miniature theory?) of which it might be a part; 7) do it while it's hot rather than reading on for full significance of the meaning, figuring that will come anyway; 8) accumulate lots of the little pieces; 9) gradually seek order and integration into larger patterns.

The latter part of this we have talked about elsewhere as collapsing outlines (Smith and Schumacher, 1972; Smith, 1979). But here, it's

the front end, early on aspect that is the item under consideration. Until this moment it had never occurred to us that the process was similar to brainstorming--get the ideas out quickly, in large quantities, and with minimal criticism or revision. We had not looked at that literature in almost 20 years (Smith and Hudgins, 1964). And we are merely raising it here, to be read, analyzed, and evaluated later. For the moment, it's a broad set of methodological hunches or hypotheses--Do brainstorming and analysis of field data have essential similarities? Can the former contribute to the better practice of the latter? Are there recent nuances in the theory of brainstorming that can be adapted, assuming the essential similarity is there? Who has done the classic work since Osborne (1957)?

3.45 The Classic Piece Gambit

Several years ago we hit upon another practical device for enhancing the meaning of our work. We call it, among other things, "the classic piece gambit." Originally it was something we seemed to be doing implicitly in our own work, later it was a question posed to students doing qualitative research. It is exemplified in two forms, depending on how much we already know. For example:

- 1) Who has done the classic piece on _____?
- 2) What do we know about _____ that _____ doesn't know?

One of the concepts which seemed important to our analysis and with which we struggled is "belief systems." Once we had gotten that far, and that's a difficult problem in its own right, then we were ready for the question in its first formatting: "Who has done the classic (and/or

most recent) piece on belief systems?" Then one gently but persistently pursues one's colleagues, students, and "betters" with the question. Shortly one is on to a good lead or two. The references from each paper leads in a step or two to most of what one wants.

In the second form, since we already knew some of Milton Rokeach's (1960, 1975) work on the open and closed mind, the question with which we teased each other was:

What do we know about belief systems that Milton Rokeach doesn't know?

This sent us back to Rokeach for a reconsideration of his theoretical framework. Beliefs, belief systems, disbelief systems, central peripheral dimensions, dogmatism, authoritarianism of the left and the right and so forth soon became part of our vocabulary.

In addition, in Rokeach we found other references. Eric Hoffer we had spent considerable time with in Anatomy. But we also found Elton Trueblood, a Quaker religious philosopher we had not read since undergraduate days; Rokeach had referenced centrally Trueblood's The Logic of Belief (1942). That turned out to be subtitled An Introduction to the Philosophy of Religion and less what we wanted and needed at that point. But, in 1942, when it was published, he raised the concept of "the naturalistic creed" and entertained a sharp critique. This was a religious phrasing of positivism which seemed to have escaped some of the later day social science critics (Fay, 1975; Bernstein, 1978; Bredo and Feinberg, 1982) and broadened that issue considerably for us. In so

doing it provided a linkage with the broader metatheoretical or paradigmatic debate which flows through, like an allegory, all six volumes of our report.

But the point here is that the classic piece gambit adds considerably to the meaning which one discovers in, brings to and shares with the initial problem phrasing and the data which are collected. How much of this outside theory (literature review?) should come early, how much along the way and how much later is another serious and difficult question. Implicitly we seem to be arguing: Know enough initially to be in the ball park, let your subjects lead you, trust your intuitions along the way, and carefully bring the classic statements in at the end to critique and to be critiqued by what you have found.

3.46 Files, Intellectual Craftmanship, and Interpretive Accounts

In his appendix to The Sociological Imagination C. Wright Mills (1959) makes a simple but several step argument which we have found persuasive. To the working social scientist, the doing of social science is the practice of a craft. He believes a need exists for social scientists to make personal statements indicating how one does what one does. And he continues, these statements should be the basis of a dialogue. And from our perspective they should lead to the development and alteration of research communities.

He then argues for keeping an intellectual file which we see as a way of operationalizing the social scientist's creativity, intrapsychic communication, and reflexivity. Paraphrasing his words, a scholar can

design a way of living and working, can form one's self as one works, can learn to use life experiences in one's work. Or, to extend the artistic metaphor, and to use Ken Beittel's terminology, it is a means of enhancing the conditions for the making of art--artistic causality, idiosyncratic meaning and intentional symbolization (1973, p. 4).

The file, journal, or set of jottings and informal essays that one makes, keeps, reads, reorganizes, in Mills' view, joins the personal and professional, integrates past, present and future of work and ideas, captures fringe thought, leads one to trust and yet be skeptical of one's own experience, builds up habits of writing, permits reflection, and the revision of plans, ideas, and opportunities and helps set a master agenda of what one is about. In sum, if true, that's a powerful set of consequences.

More than any other methodology, the field study, as we have practiced it, seems to be responsive to the kind of call sounded by Mills. In part it may well be that our movement toward a more interpretive paradigm is both antecedent and consequence to such a file. Our work in a qualitative life history mode has continued the specification and synthesis of ideas which initially appeared and were perceived as relatively independent procedures and events. Figure 4 is a move toward a more taxonomic ordering of our items which seem to make up a file much like the one advocated by Mills.

Insert Figure 4 about here

1. Interpretive Asides
2. Summary Observations and Interpretations
3. Methodological Appendices
4. Conference Statements on Methodology
5. Formalization into Book Chapters,
Essay Reviews

Figure 4: Levels of Interpretive Comments in Field Work:
An Instance of an Intellectual File

Briefly, the interpretive aside is a comment in the field notes or the interview notes in which one reaches for an insight, a slightly more general or abstract meaning of the participant's experience-near concept, or of the observer's more experience-distant concept. These occur in situ and typically are bracketed into the on going record itself.

The summary observations and interpretations are usually done immediately after one leaves the setting or the interview. We usually tape record these because of the increased rapidity of note making and because we are often driving to or from the site. The "to or from" splits the record into anticipatory ideas, plans, strategies and post observation and interview recollections and reflections. The latter are usually more substantial in amount. The contrast is often very important. Some anthropologists (Agar, 1982) have recently initiated an expectational view of the meaning of anthropological theory. In addition, as we indicated earlier, joint observing, interviewing, and later developing a summary observation and interpretation record has important idiosyncratic qualities.

Methodological appendices, statements prepared for conferences, and book chapters, journal articles, and essay reviews tend to be larger and more careful formalizations of the insights. Also they tend to be attempts to communicate more formally with students and colleagues. Community participation, if not building, as we indicate shortly is an important part of our agenda. If we knew more about the nature of crafts and guilds we would elaborate further the metaphor.

That last sentence specifies another important element of these records of interpretive comments; they contain items which prefigure future work. Someone knows a lot more about the nature, history, functioning of crafts and guilds than we do. Next time around, we will have read some of that, reflected on it in the context of the next project, and expanded our "theory of methodology" to incorporate it. In addition, we will do a related essay on artist's studios, invisible colleges, and professional societies which are sort of in the same domain but which should have some interesting similarities and differences.

We believe that Mills' ideas on intellectual files are of the utmost importance in thinking about the craft of educational research. In an elegant way he has presented a framework for ordering a number of ideas we have been fumbling with for a number of years.

3.5 Conclusion: Tacit Knowledge and Doing Better Than One Knows

This concluding section merely notes an important problem for the future. Once again we found ourselves writing about some things we had done which seemed reasonable, that is, made sense at the time, but which we did not have a clear rationale or understanding at the time. Later we made the comment that we "did better than we knew."

The experience has occurred frequently enough that it looks suspiciously like a regularity or pattern. And regularities and patterns are prime candidates for more formal social scientific analysis. From time

to time we have put labels such as luck, common sense, intuition, research artistry, and creativity on the phenomenon but these labels seem to conceal more than they reveal. The experience this time concerned the interviews we conducted and the phenomenon of "themes in lives" which appears elsewhere in the report. It was not until late in the analysis process that we perceived the linkages among the initial phrasing of the problem, the way the interviews went, the data they provided, the analytical process in reading the protocols, and the organization and formatting of the results in the final report.

At this level, the research process is a far cry from the simple descriptions in research methods text books. For instance, a recent text (Bogdan and Biklen, 1982) comments:

Traditional researchers speak of the design of a study as the product of the planning stage of research. The design is then implemented, the data collected and analyzed and then the writing is done. While qualitative studies take a similar course, the various stages are not so segmented. Design decisions are made throughout the study--at the end as well as in the beginning. Although the most intensive period of data analysis usually occurs in the later stages, data analysis is an ongoing part of the research. Decisions about design and analysis may be made together. (1982, p. 56)

It would appear that the teaching and learning of research methods remains an under-analyzed phenomenon (APA, 1958). It suggests too the continuing need for "methodological appendices." Finally, and more basically, it suggests the need for a project on the historical, personal, and organizational aspects of research. And this brings us back to analysts such as Mills, Geertz, and Stone who have helped us see better a variety of issues, decisions, and strategies of life history research.

4. LIFE HISTORY AND SOME BROADER ISSUES OF EDUCATIONAL INQUIRY

4.1 The "Larger" Problem

One of the most exciting aspects of the overall Kensington Revisited Project has been the blend of ethnographic method, historical method, and now life history method. We have pushed each of those in directions new to us, and perhaps ways new to some parts of the educational research community. Even more significantly have been the various combinations and permutations of methods: ethnography and history, and now history and ethnography with life history.

In addition as these multiple methods have blended with substantive problems and issues and with the evolution of the substantive problems a much larger domain of intellectual issues have become problematic for us. Basic assumptions, root metaphors, world views, paradigms and Weltanschauungen are the labels often used for those most basic sets of beliefs which form the very framework and context of the definition of problems, modes of analysis, and casting of solutions. Where these are open for question, in doubt, or in flux then one has a "real" problem. We have a real problem. And, we believe the educational community of researchers, theorists, and practitioners has a real problem, a real big problem.

Characteristically, we will only chip away at these "larger" problems. By chipping away, we mean sort of identifying them, sort of

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talking about them, and sort of putting some meaning into them. Each chip will start from, intertwine with, or conclude with some aspect of life history methods as we have experienced them in this part of

Kensington Revisited.

We have clustered our thoughts in three domains: life history as historical method, life history and the sociology of educational inquiry, and life history and social science paradigms.

4.2 Life History Research as Historical Method

In our presentation to the 1982 St. Hilda's Conference our intent was a look at the interdependencies of ethnography, historical method, and life history research methods. For a number of reasons that got reduced to a discussion of Ethnographic and Historical Method... (Smith, 1982). Mostly, the present essay has been on life history with a few allusions to history. However a couple of remnants in our thinking seem open for discussion here, and take us back to our purposes of a year ago.

4.21 The Brief for Historical Knowledge

Social scientists differ markedly in their concern for historical versus contemporaneous modes of analysis (Lewin, 1935). In our view this is a false dichotomy, at least theoretically. Historical events need to have left residues in the forms of beliefs, sentiments, and norms if they are to have contemporary effect. Similarly, none of these current ideas and feelings arise from the ether, they come from somewhere. Practically one might argue about the dichotomy in terms of assessment. Here we find ourselves with the historians rather than the measurement oriented social surveyors. In Milford, for instance, the

documents, the stories, the sagas of items such as the struggle to achieve multi-purpose rooms for every school in the District are very potent kinds of data. To see such a story evolve in a struggle of several years duration is to have a view with more potency than one might see in a social survey of district attitudes regarding multi-purpose rooms in elementary schools. The Kensington story of "multi-useless" rooms needs such a context.

Similarly, to have a view of the struggle to establish a district wide salary schedule, that is, to have witnessed the struggle for parity between elementary and secondary teachers, between men and women teachers, and between single and married teachers is to have an understanding that seems to go beyond the Likert sentiments of "strongly agree" to an item regarding the importance of a single salary. When the original Kensington teachers were hired, the hard won norms about these issues were violated. Fifteen years later, the teachers recalled difficulties and feelings associated with the events.

To know the history is to know also the people, the time, the place, the particular issue at debate, the arguments and counter arguments, the coalitions, the ebb and flow of debate, the compromises, the cumulating decisions and the current resolution. Without being a reductionist, one's analysis often comes down to individuals and conceptions of personality. Life histories expand the views, internally and externally, of individuals. They are an important and special kind of personality theory.

This historical stance has led us to what we have called "the longitudinal nested systems model of educational change and innovation." At its simplest it's a time line indicating the chronicle of multiple social systems treated as interdependent strands. We find it a powerful tool for analyzing educational events. Life histories speak to individual systems, personalities over time, which blend with those social systems. That's a major contribution.

This mode of thought tends to resolve for us several major metatheoretical issues which have bothered us for years. In turn, we will comment on the root metaphor of contextualism, the patterned or concatenated mode of explanation, and prosopography.

These resolutions may make little difference in the immediate substantive accounts and the concepts and theories that stay close to the data, but they indicate an internal consistency and coherence to our developing position that seems to offer a potential for larger and larger sets of educational problems and issues to be incorporated in our point of view. That we believe is very important for the power of the position to attack new issues and to rationalize complex dilemmas and conflicting points of view.

4.22 Hexter's Processive Explanation

One of the best analyses and guides to writing the life histories of educational reformers arose in reading Hexter's (1971) account of processive explanation in history. One selects and organizes one's presentation to make the essential point which one feels the data convey:

It is this outcome that warrants the substance, the structure, and the tone of the introductory section...In other words, the outcome defines the appropriate historical macrorhetoric, and the macrorhetoric in turn dictates the selection of "facts" or more accurately data to be drawn from the record. Or to put it more bluntly, amid a mass of true facts about the past too ample to set down, historians choose not merely on logical grounds but on the basis of appropriate rhetorical strategies. (1971, p. 190)

This seems congruent with our attempts to maintain the integrity of individuals qua individuals as we tended to tell each story in conjunction with a major thematic point. Also it relates to our attempts to maintain the integrity of the collectivity of the individuals as a group, for the larger point, "educational innovators then and now" and the meaning for the entire project, "innovation and change in American education."

When one gets this kind of help in the practical tasks one is trying to do, a potent kind of validation of the methodological theory occurs. This seems to be an instrumental or pragmatic criterion for the adequacy of Hexter's theory of methodology. On the surface it seems to carry more weight than the criterion of internal consistency of the point of view.

To be a bit reflexive, and more homespun, perhaps, Hexter speaks of the same phenomenon as "moving the work along":

In their piecemeal work they unconsciously accept the notion that an adequate explanation "why" is one that provides them with all the information and understanding they need in order to be moving along with their inquiry. The very moving along is a sort of "o.k." in action responsive to the query "o.k.?" that they tacitly posed before they moved along. (1971, p. 32)

But Hexter also gives help on the larger metatheoretical and paradigmatic aspects of our work. He speaks to the classical assumptions underlying social science when he contrasts his approach with the two standard views from the philosophy of history:

Hitherto we have encountered two philosophical views of what constitutes satisfactory historical explanation yielding true knowledge of the past. One, associated with the analytical philosophers Hempel and Popper, is usually described as covering-law explanation. The other, most fully propounded by White, attempts to adjust the Popper-Hempel argument derived from their conception of scientific explanation to the "story" form of much history writing; it propounds a logical schema of explanation by narration. We have also seen that these views of philosophers coincide roughly with two looser conceptions, current among historians, on the one hand that they explain the past analytically, on the other that they do it narratively. Narrative historians regularly confront the complex rhetorical problems of storytelling. Consequently, they are likely to be keenly aware of what those problems are. But because they are not by temperament analytically inclined, they have not paid systematic attention to the relation between their concern with historical rhetoric and their concern with historical truth. On the other side, most analytical historians, if pressed, would probably admit that history cannot wholly dispense with narrative, with its concern about the conjunctions of specific persons at specific moments in specific localities. They would, however, regard analysis, which obliterates or at least obscures such conjunctions, as the place to go for high octaine historical truth. Analytical historians tend to orient themselves toward what they imagine the sciences to be, and to conceive of rhetoric as an unseemly concern with mere tricks of language rather than as it is--the commitment to any coherent discourse that aims at persuasion. To them the notion that the truth-attainment of history is linked at all with the rhetoric of history is repugnant; for them, as for the analytical philosophers, truth resides only in the "facts" and their logical adumbration. Two common assumptions seem to provide the common ties, such as they are, among the four groups--two of them philosophers of history, two of them practitioners of history--that we have identified. One assumption is that historical explanation has only two forms--a covering-law form and a narrative form--there is no *tertium quid*; the other is that explanation in history deals solely with why things happened.

But, and so it seems, as always, not all philosophers of science agree with one another. Stephen Toulmin (1972) seems to be suggesting the breath of a possible synthesis:

This thesis can be summed up in a single, deeply held conviction: that, in science and philosophy alike, an exclusive preoccupation with logical systematicity has been destructive of both historical understanding and rational criticism. Men demonstrate their rationality, not by ordering their concepts and beliefs in tidy formal structures, but by their preparedness to respond to novel situations with open minds--acknowledging the shortcomings of their former procedures and moving beyond them. Here again, the key notions are "adaptation" and "demand," rather than "form" and "validity." Plato's programme for philosophy took it for granted that the functional adequacy of geometrical forms was self-guaranteeing--so that there must, in the last resort, be only one Idea of the Good, and that a mathematical one. The philosophical agenda proposed here sets aside all such assumptions in favour of patterns of analysis which are at once more historical, more empirical and more pragmatic.

(1972, pp. VII-VIII)

For us, this comes back with both short and long term time perspectives. Does the idea, the strategy, the tactic help in solving the problems of the immediate project, this time, life histories of educational innovators? Second, what does the idea or completed project have to say to the longer term research serial of which the project is one small piece? Does it integrate, conflict, expand, or perhaps illuminate the broader stream of activity?

4.23 Doing Prosopography

The day we found that we had been doing "prosopography" rather than interviewing a group of teachers rivaled the elevation in status we experienced some years ago when we found that we were doing the "microethnography of the classroom" rather than collecting anecdotal records (Smith and Geoffrey, 1968). Our concern with historical methods

led us into the work of Lawrence Stone and his recent book The Past and Present (1981). There, among other insights, we found an important linkage between the work of classical historians and what we had been calling life history methods.⁴

The introductory paragraph to his second essay, "Prosopography," overwhelmed us with its relevance to our work and its explication of how historians think about problems similar to ours:

In the last forty years collective biography (as the modern historians call it), multiple career-line analysis (as the social scientists call it), or prosopography (as the ancient historians call it) has developed into one of the most valuable and most familiar techniques of the research historian. Prosopography is the investigation of the common background characteristics of a group of actors in history by means of a collective study of their lives. The method employed is to establish a universe to be studied, and then to ask a set of uniform questions--about birth and death, marriage and family, social origins and inherited economic position, place of residence, education, amount and source of personal wealth, occupation, religion, experience of office, and so on. The various types of information about the individuals in the universe are then juxtaposed and combined, and are examined for significant variables. They are tested both for internal correlations and for correlations with other forms of behavior or action. (1981, p. 45)

Rather than take each of the ideas in his essay and spin off insights of historical thinking contributing to life history thinking we opt for a brief summary of his position and a brief commentary on its

⁴Here as in a number of places in this essay, both mentioned and unmentioned, is a debt to our colleague Arthur Wirth. He put us on to Gertrude Himmelfarb's review in the New York Times (1982). She had referenced Stone's move to Geertz' interpretive anthropology, thick description, and the Balinese cock fight, items which Smith had bantered with Wirth earlier and he was coming back in good humored jest--"Do you know this one?" Well, we didn't then; but we do now.

implication for what we have done. In part we do it this way because we came to it late. We struggled and came out where he starts. The quest becomes more one of legitimation of our efforts than hard debate.

Stone's essay breaks into eight sections, from "Origins" to "Conclusion." His initial generalization accents why we felt tacitly that our life histories were important:

Prosopography is used as a tool with which to attack two of the most basic problems in history. The first concerns the roots of political action: the uncovering of the deeper interests that are thought to lie beneath the rhetoric of politics: the analysis of the social and economic affiliations of political groupings, the exposure of the workings of a political machine and the identification of those who pull the levers. The second concerns social structure and social mobility: one set of problems involves analysis of the role in society, and especially the changes in that role over time, of specific (usually elite) status groups, holders of titles, members of professional associations, officeholders, occupational groups, or economic classes; another set is concerned with the determination of the degree of social mobility at certain levels by a study of the family origins, social and geographical, of recruits to a certain political status or occupational position, the significance of that position in a career, and the effect of holding that position upon the fortunes of the family; a third set struggles with the correlation of intellectual or religious movements with social, geographical, occupational, or other factors. Thus, in the eyes of its exponents, the purpose of prosopography is to make sense of political action, to help explain ideological or cultural change, to identify social reality, and to describe and analyze with precision the structure of society and the degree and the nature of the movements within it. Invented as a tool of political history, it is now being increasingly employed by the social historians. (1981, pp. 45-46)

If we were rewriting our research proposal we could not have focused it better.

Stone's discussions of the "intellectual roots" raises the crisis in history after World War I:

This crisis stemmed from the near-exhaustion of the great tradition of Western historical scholarship established in the nineteenth century. Based on a very close study of the archives of the state, its glories had been institutional, administrative, constitutional, and diplomatic history....

In their search for new and more fruitful ways to understand the working of the institutions, some young historians just before and after the First World War began to turn from the close textual study of political theories and constitutional documents or the elucidation of bureaucratic machinery to an examination of the individuals concerned and the experiences to which they had been subjected....

The unstated premise is that an understanding of who the actors were will go far toward explaining the workings of the institution to which they belonged, will reveal the true objectives behind the flow of political rhetoric, and will enable us better to understand their achievement, and more correctly to interpret the documents they produced. (pp. 51-52)

Not being trained in history, we found the doing of Volumes I, II, and III from this "great traditions of history" a major peak experience for us.⁵ In our work, the close study of the school board minutes, became the prototype of his position. Our life histories, in part began then with the accounts of the five superintendents of Milford and the four principals of the Kensington School. Now the perspective we are elaborating comes from the even more intensive study of the original administrative and teaching staffs. In an interesting way we blend his elitist and popular perspectives, although from a case study perspective.

Third, Stone raises three "limitations and dangers" in the nature of the data and evidence in prosopography: 1) the approach is possible only "for fairly well documented groups" (p. 57); 2) the problem of

⁵The essay History and Ethnography in the Study of Schooling (Smith, 1982) presented at the 1982 St. Hilda's Conference relates some of that feeling.

status and social rank, "At all times and in all places, the lower one goes in the social system the poorer becomes the documentation" (p. 58). The evidence "...is abundant for some aspect of human life and almost not existent for others." With our adaptations of ethnographic interviews and oral history, and the more aggressive hunt for everyone we feel we have developed data sets that meet very well, better than most historians are able to do, information on the lives of our people. In part we are replaying an earlier point "Historical method is just, nothing more than, participant observation with data fragments" (Smith, 1982).

Further, he speaks to "errors in the classification of data":

Meaningful classification is essential to the success of any study, but unfortunately for the historian, every individual plays many roles, some of which are in conflict with others. He belongs to a civilization, a national culture, and a host of subcultures--ethnic, professional, religious, peer-group, political, social, occupational, economic, sexual, and so on. As a result, no one classification is of universal validity, and a perfect congruence of classifications is quite rare....

The second danger which threatens every prosopographer is that he may fail to identify important subdivisions, and may thus be lumping together individuals who differ significantly from one another. Good research depends on a constant interplay between the hypothesis and the evidence, the former undergoing repeated modification in the light of the latter. (p. 60)

If we have a quarrel with this, it's not so much on the substance per se but the way the comment is cast. In our view, and in our experience putting meaning into our topic "educational innovators: then and now" one of the key problems, if not the key problem, was the construal of classifications of people, of characteristics, of ideas about them. The

idiosyncracies if not recalcitrance of individuals to fit or be construed into meaningful categories and patterns is what social science is all about.

His fifth point, "errors in the interpretation of data" has to do with samples and populations and the relationships between the two. Our 20 individuals are the entire population of Milford-Kensington educational innovators. Whether they are a sample of any larger population anywhere, any time, is unknown. It is our hunch, that they belong to some larger group of true believers, reformers, and innovators in education, in American, and probably western society and probably at several time periods over the last several hundred years. But, obviously, we don't know. But also, we don't believe this is a fatal flaw. Some people are arguing that the flaw might be in the nature of the reasoning (Diesing, 1971; Geertz, 1973; Hamilton, 1981).

Stone's sixth cluster of concerns are "limits of historical understanding." His three generalizations are these: 1) concentration on elite and ruling classes; 2) relative unwillingness to build into their perspective sentiment ideas, and ideals; and 3) a neglect of the stuff of politics, the play of individuals in concrete institutional setups. In summary, he comments:

Many elitist prosopographers instinctively opt for a simplistic view of human motivation, according to which the springs of action are either one thing or another....

In real life, human nature does not seem to function this way. The individual is moved by a convergence of constantly shifting forces, a cluster of influences such as kinship, friendship, economic interest, class prejudice, political

principal, religious conviction, and so on, which all play their varying parts and which can usefully be disentangled only for analytical purposes. (pp. 64-65)

Our overall theme, the natural history of true belief, and such subthemes as origins of educational ideology, educational reform as secularized religion, you do go home again, old innovators never die--and some don't even fade away--suggest that we are attempting to push through these limitations.

His brief comments under "achievements" seem a call to the possibilities of prosopography; we find the emotional appeal attractive:

If past errors can be avoided, and if the limitations of the method are recognized, the potentialities are very great. Indeed, provided that it is accepted--as it surely must be--that values and behavior patterns are strongly influenced by past experience and upbringing, the power of the method can hardly be denied. All that is needed is more willingness to recognize the baffling complexity of human nature, the power of ideas, and the persistent influence of institutional structures. Prosopography does not have all the answers, but it is ideally fitted to reveal the web of sociopsychological ties that bind a group together. (p. 65)

If we were to generalize this point, it would be to entertain a strong argument for the importance of social and cultural anthropology, and its central cluster of methods, ethnography as the broader perspective.

Here we believe our earlier study of Kensington, Anatomy of Educational Innovation (Smith and Keith, 1971) and our broad follow-up, Kensington Revisited (Smith, et al, 1982) speak to substantive issues. The methodological essays, of which this is one, in Volume VI join that issue as fully as we are able.

The last paragraph of his "Conclusion" section raises a more general integrative social science perspective with which we are in substantial agreement:

Prosopography nevertheless contains within it the potentiality to help in the re-creation of a unified field out of the loose confederation of jealously independent topics and techniques which at present constitutes the historian's empire. It could be a means to bind together constitutional and institutional history on the one hand and personal biography on the other, which are the two oldest and best developed of the historian's crafts, but which have hitherto run along more or less parallel lines. It could combine the humane skill in historical reconstruction through meticulous concentration on the significant detail and the particular example, with the statistical and theoretical preoccupations of the social scientists; it could form the missing connection between political history and social history, which at present are all too often treated in largely watertight compartments, either in different monographs or in different chapters of a single volume. It could help reconcile history to sociology and psychology. And it could form one string among many to tie the exciting developments in intellectual and cultural history down to the social, economic, and political bedrock. Whether or not prosopography will seize all or any of these opportunities will depend on the expertise, sophistication, modesty, and common sense of the next generation of historians. (p. 73)

Our only concern lies in the disciplinary place of "Education" in social science. And that can only be "solved" if we have a conception of social science and its more fundamental assumptions. If social science is in a paradigm crisis, as some believe, (Bernstein, 1975; Fay, 1975; Bredo and Feinberg, 1982) then our solution only prefaces a really bigger problem.

This discussion of prosopography seems to link life history to one important strand of history. If we had seen it sooner, it would have been a helpful model and heuristic. Getting there on our own though has its rewards, one of which is that Stone's discussion serves as a major methodological legitimation of our efforts.

4.3 Life History and Educational Research Communities

4.31 The Sociology of Educational Inquiry

Although, Kuhn (1970) is more recognized and Campbell (1979) more recent, it was Ziman's little book, Public Knowledge, An Essay Concerning the Social Dimension of Science (1968) which raised our consciousness about the sociology of science, the social relations among scientists as a key element in the nature of science. In his view science is public knowledge. Its objective:

...is not just to acquire information or to utter all non-contradictory notions; its goal is a consensus of rational opinion over the widest possible field. (1982, p. 9)

In its training programs, science is clearly social:

To understand the nature of Science, we must look at the way in which scientists behave towards one another, how they are organized and how information passes between them. The young scientist does not study formal logic, but he learns by imitation and experience a number of conventions that embody strong social relationships. In the language of Sociology, he learns to play his *role* in a system by which knowledge is acquired, shifted and eventually made public property. (p. 10)

Such ideas raise an agenda large in scope:

The "Science of Science" is a vast topic, with many aspects. The very core of so many difficulties is suggested by my present argument--that Science stands in the region where the intellectual, the psychological and the sociological coordinate axes intersect. It is knowledge, therefore intellectual, conceptual and abstract. It is inevitably created by individual men and women, and therefore has a strong psychological aspect. It is public, and therefore moulded and determined by the social relations between individuals. To keep all these aspects in view simultaneously, and to appreciate their hidden connections, is not at all easy. (p. 11)

But our purpose here is not to raise all that in some grand form, but rather to tell a story or two, a particular incident or two with a point, that bears upon the Ziman's bigger issues.

4.32 Invisible Colleges and Problems from Kensington Revisited

Sometimes it is difficult to tease apart what seem to be chance events from those events which one unconsciously is precipitating or selecting or creatively construing and causing. The Archival Case Records Conference at York, England in September of 1980 is an important case at point. Smith has had a decade long relationship with several individuals at the Center for Applied Research in Norwich, England. Among the variety of contacts and joint activities, the possibility of presenting a "Case Record" of some of his work was raised in December of 1979. The chance to return to England on a relevant and important issue made the choice impossible to refuse.

We include a paragraph from his brief two page statement "Archival Case Records: Issues and Illustrations." It shows clearly the kind of thinking which the conference encouraged. Further, one can see elements of both the substantive and methodological issues which were enhanced by the opportunity to participate:

The very preparation for the conference on Case Records requires a series of decisions which highlight many of the issues that the conference will be treating as problematic. The first of these issues might be "Which case study project, among the several with which we've been involved, should we pick?" For a number of reasons I chose our current project Kensington Revisited. We might want to explore the reasons at the conference. A second question, "Which records to include in the package of material?", is even more important and problematic. I finally decided to try and capture the entire process of the case study from the beginnings of the idea to preliminary drafts of the final report. I don't know whether

that was a wise decision, but it seems that that too, is something we need to talk about. A third decision has to do with "sampling" the totality of possible materials. So far in the Kensington Project we have several file drawers of data. We couldn't bring all of that so we sampled. A fourth issue involved "tidying up" the materials. We deliberately left some items just the way they are--handwritten, partially reworked, and so forth. Some of it may be unreadable, illegible, isolated and unconnected. And that's an interesting and important archival problem. Fifth, and perhaps our most difficult problem, we've tried to anonymize all the documents we've included. That's taken considerable time. It involves issues in the rights of human subjects, informed consent, and potential harm to the people in our settings. That issue, above all others needs discussion. Finally, these materials were collected originally "for research purposes," which is a "funny" set of underlying intentions, guiding the selection, storage and now reproduction of the items.

In a sense we were playing out a further episode with our friend and colleague Lawrence Stenhouse. He had put us on to Hexter's The History Primer (1971). Now we were countering with items illustrating Hexter's important distinction of "the first record" and "the second record." By so doing, we were implicitly asking Lawrence how his archival project would come to grips with that distinction, an idea we had not considered before.

While a number of stimulating events occurred at the York Conference, the event most relevant to this part of our continuing methodological reflections concerns the informal subgroup on History and Life History which arose out of a conversation between Smith and Ivor Goodson. This activity and decision then led to a series of other important events: 1) Direct involvement in an historical approach to case study; 2) A chance to rethink part of Kensington Revisited; 3) Meeting of several individuals intimately involved in history and life history methods; 4) Rephrasing of our interviews with former teachers of

Kensington as a mix of biographical accounts based heavily on autobiographical statements; 5) Reference to R. G. Collingwood's Autobiography and Carl Becker's critique of Henry James' Autobiography.

But most significantly, socially, it led to participation in the 1982 St. Hilda's Conference. There are boondoggles and boondoggles. Sometimes we feel guilty that running around the country and the world to national and international meetings is nothing more than simple hedonism in the pleasure of travel or an ego trip for all the rewards in notoriety, fame, and fortune. Then, occasions occur such as the St. Hilda's Oxford meetings on History and Ethnography of School Subjects.

That meeting provided a forum for a number of ideas in the relationship between history and ethnography. The formal give and take of papers, presentations, critiques, and informal discussions proved very helpful in those most critical, and difficult to do alone, dimensions of one's work--problem redefinitions, alternative metaphors, and references from domains far from one's immediate experience.

In addition it led to quite specific discussion with a number of individuals. Perhaps most specific and immediate were those with Goodson who was pursuing issues from his experience with the innovative Countesthorpe School. Illustratively, as we compared and contrasted the post Kensington faculty careers and the post Countesthorpe faculty careers the idea of the historical time in which each school ended arose as a major variable in the later careers of the staff. The Kensington teachers "returned to the world" in 1965 and 1966. In American education, the rush of President Johnson's great society conceptions and

the resources of the Elementary and Secondary Education Act (ESEA) were to spill over the land. Shortly thereafter the VietNam War and the desegregation of the schools was to intertwine in the lives of a number of our people. The teachers of Countesthorpe, in contrast, in the early 1970's moved into a world of recession, unemployment, declining enrollments, and the conservative tide of government.

This idea seemed to anchor, contextualize, and help focus the free floating cases of Kensington and our data from the lives of our 20 educational innovators and reformers. And that was very important for us.

4.33 Life Histories: "How Will It Play At St. Hilda's?"

This brief section might also have been written as a letter: "Dear Ivor, Stephen, et al." It reflects the good fortune when one is doing a report or an essay to have a specific audience in mind. As we were finishing the final analysis and writing of Educational Innovators, we were also writing various pieces of this Methodological Appendix, trying to get straight on what we had done and how it both contributed to and constrained the substantive report itself.

That all seemed to be moving along very well, that is slowly, painfully, joyously, never endingly, and so forth. Along the way, a separate, but partly related, strand of events was developing. In the Autumn of 1982, Ivor Goodson and Stephen Ball were implementing plans for a conference at St. Hilda's College, Oxford University, a year hence. The topic was to be Teachers' Life Histories and Careers. The

substantive and methodological fit seems close to perfect. Smith had been a part of the previous conference on School Subjects: Histories and Ethnographies and now was looking forward to the coming one.

The important point here is that the methodological essay while hopefully is of some interest to a larger audience could be set now into the St. Hilda's Conference as a specific target audience and the issues that have been evolving for almost a decade among a number of the individuals who are part of the conference. "How will it play at St. Hilda's" became the summary way of speaking about the impact of such a focus. In our view having an image of such an audience is a very powerful factor in shaping and finishing a piece of inquiry. If "science" is redefined as having a social dimension, then such items as this become an aspect of the conception per se.

There are several further implications for the work itself of this phrasing that makes it more than a simple truism. First, who are the people at St. Hilda's? Which ones have published within the career or life history area either methodologically or substantively? Have any worked with issues of innovation and change? Or with innovative schools as organizations? Or with the relationships between teachers and administrators? Where are they on the metatheoretical issues of positivistic or interpretive or critical stances toward education? Are they people mostly out of sociology, anthropology, psychology, or professional education? Within that mix do they have an explicit or only an implicit theory of personality, a set of ideas so important for our data and analysis.

That's when we begin to get tired. The perspective on audience, seemed to give us not just a knoll or small hill from which to see. Rather it is its own kind of range of mountains, each climbable but each requiring huge resources in time and energy for that clarification. Perhaps we can do a later revision; the first draft needs to be done next month for the final report. Or better, that's part of what the conference should be about.

4.34 Conclusion

But our major point accents the power--motivationally and intellectually--of one small group of educationists to help shape and focus our efforts. In a quite simple, mundane, concrete instance, for us, this is what all the fuss raised by Kuhn, Campbell, and others is all about.

Even more importantly, when one begins to think of educational inquiry from this perspective, all the issues of the human condition become part and parcel of the inquiry process. We believe part of the malaise in educational science (e.g. Cronbach, 1971; Jackson, 1981) reflects the dissolution of the Thorndikian paradigm and the search for a new perspective. Our life history problem, methodology, data, and interpretations and the group of people who have coalesced at the St. Hilda's Conferences are part of our quest at that level. It seems significant to us.

4.4 The Craft of Educational Inquiry

One of the not so minor misconceptions held by many students in Education is that the only practical activity in education is "teaching a self contained third gradè class" or "teaching Algebra I" to a group of adolescents. In our view, "doing research" is very much a practical activity, or in C. Wright Mills' words, the practice of an intellectual craft:

To the individual social scientist who feels himself a part of the classic tradition, social science is the practice of a craft. A man at work on problems of substance, he is among those who are quickly made impatient and weary by elaborate discussions of method and theory in general; so much of it interrupts his proper studies. It is much better, he believes, to have one account by a working student of how he is going about his work than a dozen "codifications of procedure" by specialists who as often as not have never done much work of consequence. Only by conversations in which experienced thinkers exchange information about their actual ways of working can a useful sense of method and theory be imparted to the beginning student. I feel it useful, therefore, to report in some detail how I go about my craft. This is necessarily a personal statement, but it is written with the hope that others, especially those beginning independent work, will make it less personal by the facts of their own experience. (1959, p. 195)

We have raised a number of "practical issues" in doing life history research. The substance of these and the processes by which we worked can be captured briefly in another half dozen observations, which seem to qualify as limited generalizations if not conclusions in a theory of research methodology,

First, the issues are practice oriented, to use Atkin's (1973) term, that is, they arose as we carried out our research project. They are grounded in the lived experience of the work activities and the worker as a craftsman.

Second, they are also reflexive, in the sense that we thought about what we were trying to do, we made decisions, we carried out alternatives, we reflected on what we were doing, and we tried to make continuing sense of what we were up to. And then we iterated through the process again. Obviously, the actual process was much more ragged, emotional, and lumpy than the long linear sentence implies.

Third, we perceived the issues to be an extension of our earlier use of qualitative field methods over a number of years (Smith, 1979). Lurking in the background here was one of the most important admonitions we had ever been taught. In an offhand comment, about more quantitatively oriented research, Fred Strodbeck (1964) had mentioned that in any new research project one should be using instruments that had been used before for two-thirds or three-fourths of the variables. As new instruments were included, one found out what they could do, beyond the reported reliability or validity coefficients. In keeping with the idea we believe we have gradually integrated the life history mode into our earlier modes of qualitative inquiry.

Fourth, and almost as a subpoint to the previous one, the life history mode builds upon our recent excursion into historical method (Smith, 1982).

Finally, and most importantly, as we discovered along the way our project evolved in a way that C. Wright Mills seemed to be advocating:

No social study that does not come back to the problems of biography, of history and of their intersections within a society has completed its intellectual journey." (1959, p. 6)

This essay attempts to redress the biographical, life history part of our quest.

In the course of all this, trying to finish a large and evolving project and looking for similarities and differences within and among problem statements, multiple methods, and varied theoretical perspectives, we have been trying to approach, in Education, what Mills called the sociological imagination, a quality of mind which:

...enables us to group history and biography and the relations between the two with society. That is its task and its promise. (1959, p. 6)

5. CONCLUSION:
GETTING THE METHODOLOGY STRAIGHT

In our work we have found that there is a much more complex interplay between methodology and results presented in reports than the usual description in the methodology books. In each of our projects, while there has been commonality, usually also there have been large areas of "first time through." In these first time through areas this has provoked a need to "get the methodology straight."

In this instance also we find that our methodological reflections are always being tried out in classes and seminars with graduate students learning to do field work. Part of the advice is to do short methodology and procedural papers. This often begins with an initial paragraph at the beginning of the semester which accompanies the initial problem statement before going to the field, when they think they know what it is they are to be about. Later, after the problem has evolved and they have collected data and are beginning to think seriously about the analysis and form the report, it is often propitious to write the methodology section, "to get the methodology straight."

That assignment is usually for a relatively straight forward, almost behavioral document of what it is they have done, where have they been observing, who have they talked to, what are the kinds and quantities of data, the strong and the weak spots. In effect, one tried to get an overview of what one's got and how it bears on the initial and evolving questions. This is what we mean by "getting the methodology straight."

The biggest trap to be avoided occurs with individuals, and they are more than a small minority, who find the observational exercise such a potent personal learning experience that they want to make the term paper, thesis or dissertation a revelation of personal impact, a phenomenology of the research experience. They forget that there once was a substantive problem which they were exploring and that the qualitative methodology is just a potentially potent way to to explore such a substantive problem. The issues become most complicated when the students are educationists and you, as instructor, have been urging them to develop; in the sense of extend, deepen, and articulate a "personal theory of teaching or education" which will be of use in their own teaching, supervising, administering or other roles they play in the educational process.

But the point, that we are reaching for here, in spite of our meandering about more generally, is that our volume on the twenty-odd innovators posed some very special problems that were different than we had encountered before. Each section of this larger essay involved a different aspect of the general issue, "getting the methodology straight." We had an initial review, which we reported in the methodology section of Volume V. Once we got into the project, things wiggled more than a bit. This essay hopes to have straightened some of that out. In the process of that we may have fallen into the very trap about which we caution students. We have written an overly long, involved essay on the phenomenology of life history research!

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5. CONCLUSIONS

5.1 Paradigms in Debate...

Paradigms in Debate: A Review of Knowledge and Values in Social and Educational Research^{1,2}

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This review has a stranger or more unusual quality than any other book review I have written. In a fundamental sense it represents a product of two partially independent groups, each of which provides an important context in the development of the ideas. Several colleagues (David Dwyer, Paul Kleine, and John Prunty) and I have just finished a long final report (six volumes) on our Kensington Revisited project which involved multiple methods and perspectives on a problem in educational innovation and change. Also, Arthur Wirth and I, along with eight students have just finished a year long seminar entitled "Search for a Paradigm: Education, Images of Human Nature, and the Human Sciences." In both of these experiences we wrestled with the issues raised by Brede and Feinberg. When one starts to probe educational research, theory, and practice in terms of the basic assumptions, the paradigms or metatheory, which undergird the positions, the kinds of issues which arise are fascinatingly complex, nearly impossible to integrate, and often seem incommensurate. It seems to me that this is precisely where Brede and Feinberg want to take each reader and ultimately the education community.

Briefly, Brede, a social scientist, and Feinberg, a philosopher, argue that two larger categories of ideas--knowledge and values--are at issue in two large domains of research--social and educational. Their analysis suggests three belief systems, paradigms, or sets of assumptions in which most educational and social research is couched. These are the positivistic, which is and has been the conventional wisdom for the last fifty or seventy-five years, the interpretive, qualitative, or phenomenological and the critical theory, Frankfurt

School, or neo Marxist view. The latter two paradigms have been long standing minority positions which have recently attracted an increasing number of individuals and communities.

The rhetorical style they have opted for is a brief introduction and conclusion to the overall volume and separate sections for each of the three positions. Within each of those sections they have written an introductory essay and then they present a half dozen essays, some of which are classic statements, by individuals from each of the traditions. I found this style clear and instructive. Their essays constitute a brief and important monograph in its own right.

Substantively, the key issue, is whether all this talk makes any difference in the theory, research and practice one engages in. To this reviewer, the issues are of major significance. By paraphrasing ideas of Breddo and Feinberg's as a series of questions regarding beliefs one can see the impact of the positions as they answer the questions differently. The figure is simplistic, but if you ask yourself, What is the latent curriculum I'm teaching my Ph. D. students?, then it seems that some of us are doing one set of things and some of us another.

Insert Figure 1 about here

It seems to be worth mentioning two books with similar content and content: Brian Fay's Social Theory and Political Practice and Richard Bernstein's The Restructuring of Social and Political Theory. The former is written as a more personal, less documented essay; this is the way I see it. The latter incorporates, much more heavily, lengthy quotes and excerpts from primary sources. Both emphasize sociology and

	<u>Positivism</u>	<u>Interpretivism</u>	<u>Critical Theory</u>
Do you believe...			
1. ...that educational theory and research can be value free?	Yes	No	No
2. ...that human action and human behavior are synonymous	Yes	No	No
3. ...that a simple correspondence between operational definitions and theoretical definitions is possible	Yes	No	No
4. ...that educational concepts and principles are context free	Yes	No	No
5. ...that reasons are reducible to causes	Yes	No	No
6. ...that truth is discovered (rather than constructed)	Yes	No	No
7. ...that there is a unity (and a reducibility) of human, biological and physical events	Yes	No	No
8. ...that society is a natural rather than an artifactual phenomenon	Yes	No	No
9. ...that the experiment is the prototypical social science research strategy	Yes	No	No
10. ...that the covering law model of explanation is the prototype	Yes	No	No
11. ...that equality is <u>the</u> major social value	No	No	Yes
12. ...that the basic goal of educational research is to improve the world (not just to explain or interpret it)	No	No	Yes
13. ...that the actions of the researcher are susceptible to a theoretical analysis comparable to the actions of the subjects	No	Yes	Yes
14. ...that the resolution of educational and social problems best occurs through a combined historical, biographical, and social structural analysis	No	No	Yes
15. ...that many actions of individuals are caused by social conditions over which they have no control	Yes	No	Yes
16. ...that educational theory is inextricably bound with educational practice	No	No	Yes
17. ...that a theory of knowledge and a theory of society merge	No	No	Yes

Figure 1: Beliefs and Paradigms

political science in contrast to Bredo and Feinberg's educational emphasis. That is an important difference for it raises explicitly the nature of "the good society." For the student interested in the eventual integration of the social sciences and education the three volumes complement each other very well. All three are available in paperback editions.

In a book like this, part essay and part readings, one is tempted to play a guessing game with the authors' choices of readings. For this reviewer the one strand of analysis and argument that is missing is the historical one. Hempel's 1942 positivistic paper on explanation in history, early on, became the point of attack for Dray, Gardiner, Scriven, Hexter, and others as they wrestled with the central themes of Bredo and Feinberg's book. This omission seems particularly striking in that the authors lean toward an evolutionary, historical position. In the same vein, if I had to pick alternative specific pieces I would have argued for Gergen's "Social Psychology as History," which has been another influential part of the debate. At a broader level, the stage might have been set with a passage from Kuhn. Pepper, Horkheimer and Adorno, Freire, Schutz, and Ziman would have been on my list also. Finally, C. Wright Mill's The Sociological Imagination set the stage for much of the debate in America. Their list includes Skinner, Homans, Goodman, Winch, Habermas, Gouldner, et al. I won't even try to argue a case for deleting any of those they included.

On perhaps "minor" points, this reviewer missed having an index and a common bibliographical list. Others might have appreciated a short biographical glimpse of each contributor. When a volume is to be used

as a working reference for individuals reconstructing the component beliefs in a paradigm shift, such a minor formatting change makes a major difference.

But, to return to the main conclusion, Bredo and Feinberg present a strong platform for educational theorists, researchers, and practitioners to enter and work on what this reviewer perceives to be the fundamental intellectual issue and conflict of our generation. It's a good place to start.